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ORIGINAL LECTURES.

VESICO-VAGINAL AND RECTO-VAGINAL FISTULA.

A Clinical Lecture, delivered at the Jefferson Medical College Hospital.

BY J. EWING MEARS, M.D.,
GYNECOLOGIST TO THE HOSPITAL.

GENTLEMEN: You will remember the patient now before you as the one presented some three weeks since suffering from a vesico-vaginal fistula. At that time, I made an examination of the opening, and described to you its size and position, being as large as a quarter of a dollar piece, and situated high up near the mouth of the uterus. You will recall that I spoke of the tense and thickened condition of the vesico-vaginal wall, and stated that it was a condition due to the opening in the bladder which had existed for so long a period. I alluded to the methods of treatment by the denudation of the edges and the introduction of sutures employed in these cases, and called attention to the fact that, in many cases, repeated operations were required to secure entire closure of the fistulous opening.

Since the patient visited the clinic, I have operated upon her, and I am gratified in being able to report that I have succeeded in closing the opening at one operation. Many difficulties were encountered, however, in obtaining this favorable result, and of these I shall speak now. In the first place, the day chosen for the operation was very unfavorable, so far as the proper light was concerned—"cloudy with partially clearing weather," as the Signal Bureau would state it, this condition affording a very uncertain light—one moment dark, and another fairly light, with varying changes from one to the other; not only uncertain was the light, but it was decidedly annoying. So many difficulties did this bad light introduce that a friend who happened into the operating-room strongly advised me to abandon the attempt at closure at that time. The patient, however, was so anxious to have the operation performed, and as I had been compelled, for other reasons, to disappoint her on one or two occasions, I decided to persevere and complete it. Under ordinary circumstances, I should adhere to the very good rule of selecting, as a day for an operation of this character, one in which the light was the best, and I should delay the operation if such was not the case.

The high position of the opening presented some difficulties which were increased by the absence of a good light. You can readily understand how much more difficult the manipulations would be which are required in using instruments in an operation upon a point high up in a tube, like the vagina, than those necessary at the orifice. Moreover, the canal was much contracted, and, for this reason, the opening could not be readily exposed.

I have referred to the tense condition of the anterior vaginal wall the result of the permanent opening in the vesical wall. This condition prevented the approximation of the edges of the opening without undue tension. In order to overcome this I made "relaxing" incisions, as they may be termed, through the mucous membrane, and after breaking up the connecting vesico-vaginal fascia slid the membrane up and in this manner relieved the tension. I was careful in denuding the borders of the opening to secure an ample raw surface completely encircling it, so that the surfaces apposed after the introduction of the suture would be denuded. This is of the utmost importance in order to obtain complete union. Before introducing the sutures the edges should be brought together, and a careful examination made with regard to this point.

For the purpose of introducing the sutures I employed, as is my custom, a long-handled needle slightly curved with an eye in the point. This needle I introduced about the one-sixth of an inch outside of the margin of the denuded surface, passed it beneath the tissues through the edge of the opening, carried it across the opening into the opposite edge at a corresponding position, and in the same way brought it out at a point opposite to that of entrance. It is necessary to steady the tissues with a tenaculum, and a blunt-pointed hook will assist in the passage of the needle if placed behind its point as it emerges. The needle being held in position is armed with silver wire and withdrawn, in this manner depositing the suture without difficulty. This method of introducing the sutures in these operations I regard as possessing advantages over that of using the small vesico-vaginal needles. These needles, besides being difficult to introduce, are liable to break and thus complicate the operation. In the efforts made at introduction lacerations of the tissue also frequently occur. In this case four silver-wire sutures were introduced and clamped with perforated shot. The sutures were removed on the tenth day. A metal self-retaining catheter was kept in the bladder for a period of twelve days. On removal of the catheter the patient was able to empty the bladder, the urethra being used for that purpose the first time in thirty-seven years. At the end of the second week the patient was permitted to get out of bed, and the bowels were relieved by an enema.

The gratification which the patient expresses at the favorable result following the operation is quite manifest. She describes, in a graphic manner, the sufferings she has endured in the past thirty-seven years—"scalded in the summer" and "frozen in the winter" by the constant flow of urine. Each night she retired with a basket of dry cloths beside her bed, and during the night she was kept busy in removing the wet and readjusting the dry. Now, she passes water but four times in the twenty-four hours, and enjoys a relief from suffering, the extent of which she can scarcely appreciate.

I also present to-day three patients who have suffered with recto-labial and recto-vaginal fistulæ. These lesions are caused by prolonged pressure of the child's head in parturition, direct injury, as may occur in the use of the forceps during delivery, and ulceration or abscess. Prolonged pressure and direct injury by instruments are more liable to produce vesico-vaginal fistulæ, whilst ulceration is the most prolific cause of the recto-vaginal variety. Stricture of the rectum, due either to malignant or syphilitic disease, is very frequently the exciting cause of the ulceration, and should always be considered in connection with the adoption of any surgical procedures.

Inflammation occurring between the vagina and rectum, involving the recto-vaginal fascia, may eventuate in abscess, which may open into both cavities, or the pus may gravitate towards the perineum, and

escape into the rectum, just behind the sphincter ani, and also burrow through one labium, forming a fistulous tract connecting the rectum and labium.

The symptoms presented in all of the varieties of these fistulæ are the escape of gas or fecal matter into the vagina, or through the fistulous tract into the labium. When the opening is large, the discomfort is very great, the patient having very imperfect or no control over the movement of the bowels. In simple cases, not associated with stricture or disease of the rectum, the methods of treatment are similar to those used in cases of anal fistulæ—cauterization by mineral acids and the actual cautery; nitrate of silver, either in solution or solid form, and tincture of iodine are recommended.

In larger openings between the rectum and vagina, closure may be effected by means of sutures, as in the vesico-vaginal variety, or when situated near the sphincter ani, this muscle, with the recto-vaginal wall, may be divided by the knife, or cut through with the ligature. In the case of recto-labial fistula now before us, I performed two operations—the first with only partial success. In the first operation, I employed a method described as that of the late Dr. John Rhea Barton, of this city, which consisted in the complete division of the sphincter ani laterally, and on the side of the labial opening, in this instance, as it usually happens, on the left side. The finger was introduced into the bowel, the curved bistoury carried along it and the muscle divided by cutting from within outwards. The principle of the operation you can understand, the object being to secure complete relaxation of the parts, thus permitting the walls of the fistulous tract to come in contact, and union to occur as a result. In this case, I had only partial success, owing, it may be, to a failure to divide all of the fibres of the sphincter; complete division is essential to success, and care should be taken to accomplish it. The second operation consisted in division of the wall of the fistula on the grooved director, as is practised in anal fistulæ, the parts healing by granulation.

In the two cases of recto-vaginal fistulæ, I closed one by suture after denuding the margins freely without dividing the sphincter ani. This opening was quite small, and the vaginal wall was so relaxed that no tension was exerted by the sphincter. In the other case, the opening was large enough to admit the end of the index finger, and was situated just behind the sphincter muscle. On division of the remaining portion of the perineum, it was found to contain a fistulous tract in which feces were lodged. The parts were freely cut away, so as to remove entirely the fistulous canal; the edges and borders of the opening denuded for some distance up into the vaginal tube, and the mucous membrane dissected from the inner surface of the labia, as is done in cases of perineorrhaphy, where the bowel is involved.

Silver-wire sutures, vaginal, superficial, and deep perineal—in all, fourteen—were used to approximate the parts. The quilled suture was used in bringing together the deeper surfaces of the perineum. Complete union followed in this case.

The superficial perineal sutures were removed on the sixth, the deep on the eighth, and the vaginal on the tenth day. The patient was kept on the back or side, with the limbs fastened together, for three weeks, and the bowels were opened by a large dose of oil, aided by enemata, on the nineteenth day.

In the treatment of these varieties of fistulæ, success depends in great measure, as in the vesico-vaginal forms, upon careful preliminary preparation, and, after operation, care. Where the opening is due to syphilitic disease, specific remedies should be given for some time preceding any surgical procedures.

ORIGINAL ARTICLES.

A REPORT OF A VERY LARGE SOLID FIBRO-CYSTOMA OF THE OVARY.

OPERATION FOR ITS REMOVAL; FATAL RESULT; WITH REMARKS.

BY WALTER IZARD, M.D.,
OF LYNCHBURG, VA.

MY object in reporting the following case is not so much to show anything especially unique, either in the tumor or the operation, although the great size and peculiar structure of the former would entitle it to special interest, as to attract attention to the difficulty which exists in some conditions in making a correct diagnosis as to structure, before operating, without resorting to the expedient of aspiration or tapping as a diagnostic measure.

April 15, 1881.—M. C., colored, æt. 40 (about), called on me for the purpose of having a large abdominal tumor removed. In appearance she was decidedly larger than a woman at the full term of utero-gestation; general health fairly good, although she was losing flesh rapidly according to her own statement. She said the tumor had first began to attract her attention about three years previously, as a very slight enlargement in the right inguino-iliac region, that its growth had been very slow until the past ten months, since which time it had been rapid—nearly all of its present enormous size had been gained within that time; the tumor had never been painful until lately, when she had suffered much from the effects of its weight, having constant backache, sharp pains in thighs and abdomen, great irritability of the bladder, hemorrhoids, alternate diarrhœa and constipation, etc. The menstrual function was still active, regular and normal in quantity and duration. Of late she had suffered much from œdema of the lower extremities, locomotion had gotten to be difficult and laborious, and she was incapacitated by her size and sufferings from pursuing her daily avocation, upon which she was dependent for a living. She urgently requested its removal.

Physical Examination.—The abdomen, upon inspection was ovoid in shape; upon palpation the tumor appeared to fill the entire abdominal cavity, its fundus reaching to the ensiform cartilage and encroaching upon the diaphragm to the embarrassment of respiration; it communicated a highly elastic sensation to the touch; *fluctuation was distinct* and unmistakable; careful percussion and auscultation confirmed the above indications of its size and character. The uterus, upon examination, was *in situ* and healthy, the sound introduced showed the extent of its cavity to be three inches (she had borne children).

Dr. John W. Sale, of Liberty, Va., then my partner in the practice of medicine, was called in consultation and agreed with me in the diagnosis, which was cystic tumor of the right ovary. Thinking the case a favorable one and conceiving that nothing was to be gained by delay, I agreed to remove the tumor, and a day in the following week

was fixed for the operation; in the mean time the patient was put upon the preparatory treatment usual in cases for ovariotomy, and all the arrangements as to cleanliness and general hygiene about the room to be occupied by the patient were strictly attended to. A second examination previous to the time set for the operation confirmed my opinion on all points.

April 25th, at 12 M., I proceeded to operate in the presence of and assisted by Dr. John W. Sale, Dr. E. W. Sale, Dr. J. C. Brown, and Dr. F. Sale, then a medical student. The usual incision about three inches in length was made between the umbilicus and pubis in the median line; upon opening the peritoneum a considerable quantity of straw-colored serum escaped and the tumor appeared in the incision red and fleshy in appearance and very firm and elastic in feel. The trocar plunged into it in two places obtained nothing but a few drops of venous blood; there was of course no diminution in its size; the incision was then prolonged upwards by the side of the umbilicus and the hand was introduced to examine its attachments; it was found to be free from adhesions anteriorly, but posteriorly it was closely adherent throughout the whole of its extent, the adhesions involved the sheath of the aorta, the common internal and external iliacs, the vertebral column, and the whole posterior aspect of the abdominal cavity; there were no adhesions to the bladder or uterus; it arose from a very stout, fleshy pedicle on the right side in the region of the ovary. The tumor was then with caution and great difficulty peeled off from all its attachments and turned out of its bed; this was accomplished without injury to any of the important vessels with which it was in relation. Two of the gentlemen assisting me then held it up, with difficulty, on account of its size and weight, while I transfixed the pedicle and tied it in two parts with a ligature prepared of twisted saddler's silk; the tumor was then cut off. The pedicle, which was very short, was about two inches in diameter, and was found to consist of a perfect mass of bloodvessels simply bound together by connective tissue of a fibrous character. The arteries varied in calibre from the size of the brachial artery to that of the most minute arteriole. Owing to the dense and unyielding character of the pedicle the stout double ligatures thrown around it were found totally inefficient to control the hemorrhage from these numerous vessels. Other expedients were tried, but the same cause prevented any means of compression in mass or in subdivision from succeeding, so that it became necessary to secure each vessel separately, which was done; most of them were ligated, while torsion was practised with a few. Very little blood was lost, comparatively, during the operation, the gentlemen assisting me rendering valuable aid in compressing the pedicle during the ligation of the vessels. The peritoneal cavity was then carefully sponged out, and the wound, which it had been found necessary to enlarge so that it finally extended nearly from the ensiform cartilage to the pubis, was closed with silver wire sutures including the peritoneum, alternated with silk sutures involving only the skin and cellular tissue; the pedicle, with its bunch of ligatures, was secured in the inferior angle of the wound. The patient, whose pulse had continued good throughout the operation and whose surface was pleasantly warm, was then put to bed, warmly covered, and surrounded with bottles containing hot water. She rallied promptly from the chloroform and expressed herself as feeling moderately comfortable, though she complained constantly of thirst. The operation occupied about an hour and a half.

The Tumor.—Weight forty pounds. Upon section was found to consist of layers and bundles of fibres eccentrically arranged, running in different directions, but at somewhat regular intervals gathering themselves into a concentric shape, and running round a small cyst. These cysts, dispersed with considerable regularity throughout the tumor, varied from the size of a walnut to that of a small bullet, and uniformly contained a homogeneous gelatinoid substance; the portion of the tumor immediately surrounding these cysts, occupied by the concentric laminæ, was pale and poorly supplied with blood, while the remainder of the tumor was enriched by a most abundant circulation.

Just at the point of entrance of the pedicle into the tumor, a smaller tumor, about the size of a large orange, was attached to the larger one by a short and fleshy pedicle.

The patient died during the ensuing night from shock. The apparent rally after the operation was not maintained—thirst increased, jactitation came on, succeeded by symptoms of exhaustion, and these were rapidly followed by collapse and death; a result to be expected from the character of the tumor and the enormous amount of pressure that was suddenly removed from the vessels and nerves of the abdominal region, besides the large amount of blood which was actually removed from the circulation in the removal of the tumor.

Remarks.—It is, perhaps, needless for me to say that if the nature of this tumor had been made out before operating, the operation would not have been performed. The object of this paper is to call attention by adding this to the record of similar cases heretofore operated on by others under similar misapprehension, to the fact that the mistake made in this case is one easily made under similar circumstances, not only by the inexperienced, but by those who are careful to use all the means in their power to clear all points of doubt before resorting to so grave an operation, as even a favorable ovariotomy would be. In this case the tumor filling the abdominal cavity, and giving to the external appearance its oval form, was uniformly covered to the depth of about half an inch with the serous fluid effused in the peritoneum as a result of pressure—this fluid communicated the sense of fluctuation in examining the tumor, and alteration of the position of the patient produced no gravitation of the fluid to dependent parts, because of the complete filling of the abdominal cavity with tumor and exudation. The patient was not tapped or aspirated, a measure that would have pointed out the true nature of the tumor, in obedience to the opinion held generally,

I believe, by ovariotomists, that this measure is not advisable in cases where an operation is contemplated, and where the other signs of an ovarian cyst are sufficiently clear as to leave no reasonable doubt as to the nature of the tumor. I think it will be agreed that this was the case in this instance.

Emmet, in his *Practice of Gynæcology*, records a case of a woman applying at the Woman's Hospital, New York, with a tumor for removal, which he at first suspected to be a uterine fibroid, but the woman being seen and examined subsequently by Drs. Sims, Thomas, and Peaslee, and they holding the opinion that it was an ovarian cyst, he became convinced that they were right, operated for its removal and found it a uterine fibroid. The patient died from shock in a few hours. I cannot cite the page where the account of this case can be found, as I have not the book by me, but it is in his article on "Ovariotomy" in the edition of 1878 or 1879 of his valuable work on Gynæcology.

In the AMERICAN JOURNAL OF THE MEDICAL SCIENCES for October, 1871, page 433, Dr. Packard reports to the College of Physicians of Philadelphia, a case diagnosticated as ovarian cyst by himself and others, in which he proceeded to operate for the removal of the tumor; but, having introduced the trocar into the uterus, under the impression that it was one of the cysts, and having therefrom some troublesome hemorrhage, he secured the bleeding points, and then, considering the removal of the tumor impracticable, he desisted from the operation, and closed the wounds, uterine and abdominal. The patient, however, had symptoms of peritonitis and metritis, and died on the 14th day after the operation. An autopsy was made, and he says of the tumor that it "was fibrous and solid, and presented a most deceptive fluctuation; it seemed to have been developed from several centres, chiefly, perhaps, from the left ovary; a strong fibrous pedicle connected it with the upper and back part of the uterus. In the centre of one fibrous mass was what seemed to be a shrunken cyst. The whole tumor was judged to weigh thirteen or fourteen pounds," etc.

In THE AMERICAN JOURNAL OF THE MEDICAL SCIENCES for July, 1871, page 157, Dr. Walter F. Atlee reports to the College of Physicians of Philadelphia an operation performed by him on a woman fifty-two years old for the removal of a tumor about the size of the adult head, under the belief that it was a cyst of the right ovary. The tumor proved to be a uterine myoma, which nearly obliterated the cavity of the uterus; it weighed five pounds. This operation was really one of extirpation of the uterus. The patient recovered without a bad symptom.

I have within my own experience known two other cases, which I can only refer to, as they have never been published. One was in a western city, and multiple subperitoneal uterine fibroids were removed from a lady, under the impression that the tumor was a multilocular ovarian cyst, resulting in death from shock in a few hours. The operator was a distinguished ovariotomist from one of the large eastern cities, who went west for the purpose of operating. The other case occurred in one of the counties adjacent to this city, and was operated on by a surgeon of the section where the lady lived. This was also a fibroma of the uterus, and death resulted within forty-eight hours; cause unknown.

I do not wish to be understood as attempting to give the complete bibliography of what may be termed these mistaken operations. Suffice it to say that, since this operation, I have found record or reference to about a hundred of these operations under mistaken diagnosis. My object is simply to add to the clinical record of them, and also by means of such addition to set forth the need of the greatest caution in diagnosis. Tapping, when it is performed, ought to make the case clear, but even this sometimes fails, as it was performed in Dr. Packard's case without a diagnosis of fibroid being made.

My own case has some points of peculiarity as compared with the others. It was the only one, except Dr. Packard's, in which the tumor arose from the ovary, all the others being fibroids of the uterus; also it was the largest that has come within my observation or reading, and, lastly, its structure was, I believe, a very unusual form of ovarian disease. I regret that I cannot add a report of a microscopic examination of its minute structure, but none was made.

REMARKS ON SOME OF THE RELATIONS WHICH ANIMAL HEAT AND NERVE FORCE OR MOTION BEAR TO EACH OTHER.

BY ROBERT BIRD, M.D.,
OF COBHAM, VA.

HEAT and nerve force—are these mutually convertible? Light, when it impinges on the retinæ, becomes physiological force;[1] sound may, when it falls on the auditory nerve, induce neuralgia—that is, physiological changed into pathological motion;[2] while words conceived in the brain may become physiological motion in the mouth and hollows of the throat, physical force in the atmosphere, and again physiological motion in the auditory nerve and the brain, as happens when we select and speak words to produce an echo.[2] This being the case, it is not unreasonable to suspect that heat may, under certain circumstances and conditions, become directly transformed into nerve force or motion, and, *vice versa*, that nerve force may be transformed into heat. By directly transformed is meant that the one is changed into the other directly as heat is changed into electricity in the thermo-electric pile, and not through a chemical or any other medium.

In the processes which constitute artificial egg-hatching, we have, at the touch of heat, an instance of development of lowly organized matter into an animal in which the higher displays of life are manifested. It has not yet been experimentally demonstrated that in these processes heat is directly transformed into nerve force or motion, but that it is so appears to be assumed by at least one of our

[1] See the author's paper in Indian Medical Gazette, Calcutta, November, 1880, "On the Retinal Spectra."
[2] On Sound as a Cause of Disease, *Idem*, April, 1874.

leading physiologists.[1] When this reasonable assumption has been strictly verified, it will then clearly appear that nerve force takes its place in the circle of the forces, and does its share of work in the general round. The warmth of the egg-hatcher leads to the hatching out of fully developed chickens, which are capable of doing all that chickens hatched in the ordinary way are capable of doing. They eat and grow, move about, and deal as intelligently with their surroundings as chickens usually do, and in time they may produce eggs, and hatch them in the natural manner. Here we have artificial heat, nerve force, animal heat, and nerve force following each other in orderly succession, the result of which, as these operate on organic matter, is a complex physiological display, constituting a fowl and its life. In the course of this physiological development, we see, on the one hand, that the cell or egg has certain tendencies and capacities which compel and enable it to develop in certain directions, within certain limits, and after certain patterns, and, on the other hand, we see the numberless and far-reaching operations of the nervous force ministering to the growth, the maintenance, and general well-being of the body. Heat in these processes can scarcely take the place of these tendencies and capacities, but it may reasonably be concluded that it is, in some way, and to some extent, changed into the nerve force, which, as it were, carries out their mandates.

Chossat,[2] in the course of his experimental researches into the effects of starvation on animals, found that these continued to lose in temperature as the starvation was continued, and that when the loss had reached a certain limit (varying in the individuals experimented on), the animals suddenly died. But he also found that, when this limit was nearly reached, and the animals about to die, if he applied external warmth the dying creatures at once began to recover the use of their senses and their muscles, and took food when it was offered to them. Further, if the application of artificial warmth was continued long enough, they digested the food, gained in temperature and strength, and soon again became independent animals; but if it was withdrawn too soon, they relapsed and died. The restoration to life in these experiments which followed the application of heat was, I think, too sudden and quick to admit of any other conclusion than this, viz., that the heat became directly transformed into nerve force in the nerves and nerve centres, which, permeating the dying tissues, maintained vitality till the restored sustenance had been digested and assimilated.

John Hunter[3] instituted some experiments to test the power of living bodies to resist the effects of heat and cold. In one of these he immersed a dead and a living penis in warm water, and carefully measured the rise of temperature in each. He says "the living and dead parts, being both immersed in water gradually made warmer from 100° to 118°, and continued in that heat for some minutes, the dead part raised the thermometer to 114°, while the living raised it no higher than 102½°. It was observed by the person on whom the experiment was made that after the parts had been in the water about a minute, the water did not feel hot; but on its being agitated it felt so hot that he could hardly bear it. Upon applying the thermometer to the sides of the living glans the quicksilver immediately fell from 118° to about 104°, while it did not fall more than a degree when put close to the dead; so that the living glans cooled the surrounding water to a certain distance."

At first it appears as though the dead penis had taken in more heat than the living penis, but that this is only in appearance is indicated by the living glans cooling the surrounding medium from 118° down to 104°, so that this (the living glans) not only absorbed the 14° missing from the water in contact with it, but its own special share of heat also. What did it do with the missing heat? That is the 14° added to the 11½° which marked the difference between the temperatures of the dead and living parts. That it absorbed them is certain, but that it did not continue to retain them in the form of heat is equally certain, as the applied thermometer only registered a temperature of 102½°, while the surrounding medium stood at 118°. Nor can it be concluded that it conveyed them away by conduction, for heat travels much too slowly among the living tissues by conduction to admit of the phenomenon being explained in this way. By a process of elimination we are almost driven to conclude that the missing heat, having been transformed into nerve force, was, in this form, transferred to the nerve centres. The above simple experiment, made by so exact and eminent a man as John Hunter was, will bear a good deal of thinking over. In explaining the restorative effects of heat on animals rendered insensible by exposure to cold, or the increase in the mortality, which invariably occurs among the very young and very old, when the weather suddenly becomes very hot or very cold, a chemist would use the principles of chemistry, and a physicist the principles of physics, while a physiologist, if he would arrive at the truth, must use all the principles, whether these are mechanical, chemical, or physical; and when, on the one hand, he teaches that the force which fills the brain and other ganglia is mainly produced chemically through the oxidization of the nerve substance, he may, on the other, admit that in some instances and to some extent it is derived directly from heat, and that in others it is reconverted into heat, and as such radiated into the colder space of the surrounding atmosphere. The capacity of the nervous system, however, is limited in dealing with heat impinging on it. When acted on by more than it can dispose of, then death results—the death of a part if the application be local, or death of the whole body if the application be general, as instanced by burns and scalds ensuing on contact with hot water and hot metals, and by the occurrence of *coup de soleil* and heat apoplexy—the result of exposure to the sun's rays in tropical climates.

[1] Carpenter. See his Human Physiology, ch. i.

[2] Experimentales Recherches sur l'inanition, and Todd and Bowman's Physiology, vol. ii. ch. xxx.

[3] Palmer's edition of his works, vol. iv. pp. 140, 141.

"When the doctrine now known as that of the conservation of energy was first worked out in regard to the production of animal force, it was supposed that every exertion of muscular power involved a metamorphosis of the tissue of the muscle itself sufficient in its amount to produce the mechanical equivalent of the heat that would be otherwise liberated by the combustion of its hydrocarbon. But the observations of MM. Fick and Visliscenus on the increase of urea and other nitrogenous products of the disintegration of the muscles which shows itself in the urine during and after prolonged muscular exertion, confirmed, as they have been, by the results of many similar inquiries, seem to place it beyond doubt that the amount of motor energy put forth is so completely out of proportion to that of the muscular tissue, which we can suppose to have been consumed, that we cannot do otherwise than attribute it in great part to the oxygenation of the non-azotized components of the blood during its passage through the muscle."[1] As the question is still an open one, the discrepancy above alluded to might also be explained by affirming that, in muscular contraction, the nerve force poured upon the muscles is transformed into motion, as heat or electricity is in moving machinery, and that the excreted urea is merely a measure of the waste of the muscle doing the work, and not of the force by which the work is done. This statement is supported by many other phenomena, and scarcely contradicted by any. During and after the convulsions of epilepsy, and the exacerbations of the spasms in tetanus, the temperature of the body rises. In epilepsy, I have seen the temperature rise from 99° to 104°, and in tetanus it has been noted as high as 110°.[2] To all appearances, there is in these diseases a sudden and violent overflow of nerve force from the centres upon the muscles—an overflow which is too excessive to be wholly used up in the ordinary way. The muscles convert the larger proportion into violent motion, while the remainder is converted into heat through resistance, just as electricity is converted into heat in its passage through platinum. It does not ever appear that muscles are capable of contracting without the application of some stimulus, while it does appear that the ability of the muscle to contract at all depends on its tissues being maintained in a certain condition. But in debating this question, it is necessary to separate the instrument and the force which moves it. Brown-Séquard's experiments[3] show how essentially necessary a sufficient supply of arterial blood is to the maintenance of contractility, but they do not indicate that the blood furnishes the motive power. Nerve force is the natural motive power of muscle, but some other forces may be substituted for this in producing muscular contraction, as, for instance, physical contact. It must, however, be noted that these forces, or stimuli, come from outside the muscle, and are not inherent in it or a property of it.

Pathology in many of its phases supports the statement that nerve force is directly convertible into heat. The rise of temperature in ague does so. In the beginning of this disease the patient complains of feeling very cold, and shivers. Yet simultaneously with the coming on of the shivering the temperature of the skin begins to rise, and in the course of a few minutes, and while the patient is still shivering, the skin has grown burning hot. It appears to be almost necessary in this instance, in the absence of any other satisfactory explanation, to attribute the sudden rise in temperature to a sudden metastasis of nerve motion from the centres upon the periphery. For it can scarcely be attributed to an increased flow of blood through the capillaries, and to an increase in the disintegration or oxidization of the tissues consequent on this; because there is no such increase in the blood flow, as is proved by the skin remaining blue and pinched, and that there is as yet no increase in the generation of carbonic acid in the tissues seems also to be proved by the labors of Lehmann,[1] which go to show that the amount of carbonic acid from the body is lessened and not increased in intermittent and other fevers. The trembling or irregular action of the muscles also indicates that a metastasis or overflow of the nerve force is in progress. Through what channels does the force or motion flow? Probably along or through both the afferent and efferent nerves, for the paradoxical experiment shows that the same nerve motion may travel along both.[2]

In proving that the generation of animal heat is at any rate not altogether dependent on the oxidization of carbon during respiration, or necessarily on oxidization of carbon at all, Sir Benjamin Brodie adduces the following cases: "A man met with an accident which occasioned a forcible separation of the fifth and sixth vertebræ of the neck attended with an effusion of blood within the theca vertebralis and a laceration of the lower part of the cervical portion of the spinal cord . . . the patient in this instance died at the end of twenty-two hours from the period of the injury, and for a considerable time previously to his death, breathed at very long intervals, the pulse being weak and the countenance livid. At last there were not more than five or six inspirations in a minute. Nevertheless, when the ball of a small thermometer was placed in the inside of the groin the quicksilver rose to 111° of F. Immediately after death the temperature was examined in the same situation and found to be still the same."[3] Is it unreasonable to conclude that in this and similar cases the heat was generated by an excessive flow of nerve motion towards the periphery and the direct conversion, through resistance, of this into heat on the skin and

[1] Carpenter's Human Physiology, Smith's Edition, pp. 37, 38.
[2] Edward's Article, Animal Heat, Cyclopædia of Anatomy and Physiology, vol. ii.
[3] Journal de la Physiologie, 1858, vol. i., and Carpenter's Physiology, ch. xv.

[1] See translation of his works for the Cavendish Society or the American translation.
[2] Paradoxical contraction of muscle.
[3] Physiological Researches, pp. 120–121; and for a somewhat similar case see *Lancet* (London), March 6, 1875, detailed by Dr. Teale.

outer tissues of the man. That this idea had at least partly formed itself in the mind of Brodie is evident from his further remark, "there seems to be no reason why the evolution of heat should not be one of the results of the operation of the nervous power as it is of electricity."[1]

Dr. Dowler, of New Orleans, observed that the temperature of the bodies of those who had died from yellow fever often rose to a remarkable height immediately or shortly after death. In one instance just before death the temperature was 104° and fifteen minutes after death it had risen to 113° in an incision of the thigh.[2] It is very noteworthy that Dr. Dowler almost invariably found that the lowest temperature was to be met with in the brain. A corresponding rise of temperature is often observed in the bodies of those who have died from Asiatic cholera. In this disease, as is well known, one of the leading symptoms is great depression of temperature. It is not unusual to see this fall, as the disease progresses, from the normal standard to 85°, 80°, and even lower in the cold season, while after death it will often rise to 100°, 102°, or 104°. Now how are these post-mortem rises in the temperature to be explained? They cannot result from the oxidization of carbon during respiration, for respiration has ceased, nor from the processes of secretion, assimilation, or nutrition, for these also have ceased, nor from muscular contraction, for the rise is often an accomplished fact, before the slightest trace of rigor mortis is apparent. But can these processes, when in full operation combined with muscular contraction, and with the heat of fever thrown in to help, can they generate so high a temperature as 113°? The vital or physiological processes generate a temperature of from 98.5° to 101° according to the locality examined, while combined with fever they rarely, if ever, produce a temperature of more than 108° or 109°, but in Dr. Dowler's cases the post-mortem temperature exceeded the fever-heat, and in one instance when the fever-heat rose to only 104° the post-mortem temperature rose to 113°. I think we must attribute this remarkable rise to some other source than the after-death contraction of muscle. Can it reasonably be attributed as has been done in other cases—in cases of tetanus and injury to the spinal chord—to a metastatic flow of the nerve force? Water, when freezing gives out heat; is it permissible to say that the nerve centres when dying give out their special motion in copious flow along the nerves, and that, as this cannot now be utilized in carrying on the processes of life, it is necessarily converted into heat by collision with the dead tissues? In cases of death, after lingering illness, attended with much nervous exhaustion the post-mortem rise in the temperature is slight, if met with at all.

Bernard's[3] celebrated experiments on the sympathetic seem also to support the theory that nerve force is directly transmutable into heat. This physiologist showed that section of the sympathetic trunk in the neck was invariably followed by a rise in the temperature of the cheek on the corresponding side of the body, a rise of as much as 11° above the temperature of the other side. He further showed that excision of the superior cervical ganglion or section of the cerebro-spinal nerves of the face on the same side did not materially change the result of the experiment. Now, how is this rise to be explained? By attributing it to an increase in the circulating blood of the heated parts? This explanation can scarcely be received, for Bernard tied the vessels and yet the high temperature continued. Moreover, the temperature of inflamed parts never approaches 11° above the normal standard; but in Bernard's animals the high temperature continued for months without the supervention of inflammation or any other disease. While in Sir B. Brodie's experiments on animals under the influence of woorara, which dilates the capillaries, it was shown that the temperature declined while the heart was beating steadily.

Dr. Carpenter,[1] while attributing the rise to an increase in the flow of blood through the parts, seems to be conscious of the insufficiency of the theory, for elsewhere, he says, "in some instances, however, such variations seem to be independent of changes in the supply of blood. Thus Schiff has shown that irritation of the sensory nerve in a dog under the influence of woorara and therefore incapable of performing muscular movements, causes increase of temperature of one or other of the hemispheres of the brain, and was independent of changes in the circulation since it occurred, even as late as twelve minutes after the heart's action had ceased. Even irritation of the nerves of special sense will produce this effect, for it was distinctly perceptible in a thoroughly woorarized dog, before which an umbrella was suddenly opened." If then a change in the circulation or respiration cannot explain the rise of temperature in these instances, why should it be adopted to explain the rise in Bernard's experiments when it cannot possibly explain it? and if transformed mechanical force (irritation) in the case of the one dog, and transmuted light (reflected from the umbrella) in the case of the other produced the rise, why should it not be received that conversion of nerve force into heat produced it in the animals operated on by Bernard? Nerve force supplied by the ganglia *directly before* the excision of the superior cervical ganglion and section of the cerebro-spinal nerves, and indirectly by anastomosis after the section and excision. The removal of one ganglion lessened the source of nervous supply; it lessened, but did not suppress it. The reader, who in thought, is not familiar with the transformations of force within the tissues may read the following with advantage: "A patient was for some time in the London Hospital, in whom there was such an undue impressibility of the retinæ that she could not remain in even a moderate light without a continual repetition of the act of sneezing." In this instance the physical motion named light became in the tissues of the eye and brain transformed into physiological motion muscular and other.

[1] Op. cit. p. 127. [2] Todd and Bowman's Physiology, 425.
[3] Recherches' Expérimentales sur la Grande Sympathetique, Paris, 1854.

[1] Human Physiology, Smith's edition, p. 546.

In reviewing the facts above adduced it seems difficult to deny that there is some ground for the belief that heat, under certain circumstances and conditions, becomes directly transformed into nerve force or motion in the nervous tissues. The parallel instances of sound and light becoming nerve motion in the organs of sight and hearing, the phenomena of egg-hatching by artificial means, and of the restoration of animals senseless from excessive cold under the application of external warmth, as also of heat accelerating and cold retarding the development and growth of vegetables, and of the lower animals, the action of heat on hibernating animals, the experiments of Chossat illustrating the effect of starvation on animals, and above all the experiments of John Hunter on the absorption of heat by dead and living tissues collectively and individually support the theory that heat is directly transmutable into nerve motion. While on the other hand the theory that nerve motion is directly transmutable into heat is supported by the rise of temperature, in tetanus and epilepsy, in intermittent fever, in some cases of injury to the spine, as for instance that described by Brodie, in the bodies of those who have died from yellow fever, cholera, and other acute diseases, and in the cheek after section of the sympathetic trunk in the corresponding side of the neck.

I am not arguing to show that heat is the main source of nerve motion, or that nerve motion is the main source of animal heat, but only to show that there are facts which indicate or, at any rate, support the hypothesis that heat is one of the sources of nerve motion, and nerve motion one of the sources of heat. That nerve force is one of the sources of animal heat appears to me to be taught more or less distinctly in the writings of Le Gallois, B. Brodie, Schiff, Bernard, and Prof. H. C. Wood, but the general opinion is that animal heat is exclusively derived from chemical sources. Physiology is, and always has been, colored by the prevailing science of the time, and in this way its history is the reflected history of general science. In the course of its development, it has been metaphysical, mechanical, and chemical, while now its constitution is getting to be colored and invigorated by the adoption and assimilation of the principles of physics. That it should be so is unavoidable—nay, it is right—for physiology, in its completest form (if it ever attain to such perfection), must be the sum and embodiment of all sciences, and if we would find the truth, we must use the principles and facts of all sciences in explaining and harmonizing vital phenomena. No one saw this more clearly than Liebig, himself one of the creators of animal chemistry; so he expressly cautions physiologists against the misuse of chemistry in these words: "From the false notion which has been formed of the influence of chemistry on the explanation of the vital phenomena, it has happened that, on the one hand, this influence has been estimated too low, and that, on the other, the expectations and demands on it have been raised too high."[1]

[1] Animal Chemistry, 3d Edition, p. 252, Gregory's translation.

A CASE OF TRAUMATIC AMAUROSIS.

BY OLAF PAGE, M.D.,
OF VALPARAISO, CHILI.

THE patient, aged 40, a porter by occupation, gave the following history: Two months ago, whilst carrying a load down some cellar-stairs, he missed the last step, and fell violently upon the floor, striking his face and forehead over the left eye. He did not lose consciousness, nor did he have any vomiting. On the fourth morning after the accident, he awoke with his eye very much swollen, and with total loss of sight. He had a leech applied, and a week later applied for advice. I found both upper and lower eyelid so swollen that it was impossible to open the eye. The upper lid was of a dark, livid color, and, with the exception of a small portion in the middle, intensely hard. The ecchymosed conjunctiva protruded between the lids. I made a small incision into the soft spot, giving exit to a little pus. He was given a weak carbolic-acid lotion to apply to his eyelids. In a week or ten days, the eye had become soft enough to enable me to separate the lids. The conjunctiva, swollen and hard, had become attached to the margin of the lower lid, so that I was obliged to separate it with a knife. On examination, the aqueous humor appeared very cloudy, and the pupil irregular. The movements of the eyeball were pretty free in all directions. A solution of atropine, one grain to the ounce, was prescribed, to use but once a day only, as he complained of no pain.

As the swelling went down, and the aqueous humor became less turbid, I discovered a posterior synechia on the nasal side of the iris. The rest of the pupil was apparently quite free. Five weeks from the time of the accident, I examined him with the ophthalmoscope, and found a complete atrophy of the optic disk. Not a single bloodvessel was to be seen upon its surface. I could find no hemorrhages in any part of the retina.

The right eye the patient had lost ten years previously, probably from gonorrhœal infection. The stump, however, was entirely painless on pressure.

As the patient never suffered from any brain symptoms, the mischief was probably limited to the orbit alone, the blow having fractured the orbital plate, which, with the extravasated blood, must have cut off the blood-supply to the optic nerve.

VALPARAISO, June 13, 1883.

MEDICAL PROGRESS.

PROPHYLAXIS AND TREATMENT OF CYSTITIS IN WOMEN.—PROF. KÜSTNER considers that many cases of cystitis in the female are due to a transfer of septic material from the vulva into the urethra and bladder by means of the catheter, and recommends that catheterization should not be done by touch alone, but should be aided by the eye. Further, the vulva should always be carefully cleaned before catheterization. It is highly important to have a clean instrument, and he considers this impossible with the instrument generally used. Even when it is boiled in carbolized water and cleaned with a brush, some material, more or less septic, will be found adhering to the eye,

which cannot be reached by the brush, even when the catheter has a perforated extremity. He recommends a simple, straight glass tube of the size of an ordinary catheter, with a bevelled opening for introduction into the bladder. Küstner says that the curve of the female catheter does not correspond to the normal course of the urethra, which has been found by vertical sections to be either straight or sometimes S-shaped, never C-shaped, and consequently a straight tube will answer perfectly well. These catheters have been in use in Küstner's clinic for a long time, and no case of vesical catarrh has occurred since they first began to be used, although some patients have been catheterized as many as forty times after severe operations. The bevelled edge of the catheter is ground very smooth, and of course a catheter with a chipped place in the end must be thrown aside.

Local treatment, washing out the bladder, is the best means of treating vesical catarrh. The ordinary instruments in use for this purpose, double-current catheters or T-tubes with India-rubber tubing, are insufficient for this purpose. Küstner uses a glass funnel-tube, somewhat like his catheter, into which a hollow stopper fits. When the bladder is full, the stopper is removed, and the urine escapes. For irrigation, he formerly employed weak carbolic solutions, but he now uses corrosive sublimate solution, 1 to 5000, giving not more than two irrigations in a day. By this means, severe cases of cystitis have been cured in two weeks. This funnel-tube and stopper are made by Otto Kloppe, Johannisplatz, Jena. [It may be pertinent to inquire why the stopper should be hollow if it must be removed when the urine is drawn.]—*Deutsche med. Wochensch.*, May 16, 1883.

NUSSBAUM'S TREATMENT OF CANCER.—PROF. NUSSBAUM noticed, several years ago, that indolent ulcers of the leg rapidly improved when an incision was made around them, at about a finger's width from their margin, and down to the fascia, the incision being prevented from healing. Not long ago he treated a case of cancer of the mamma, with so strong a tendency to hemorrhage, that each dressing of the ulcer threatened her life. As the patient was nearly moribund from the frequent loss of blood, Nussbaum surrounded the tumor with a strong subcutaneous ligature, which he drew together with all his strength, and then tied. The patient rallied completely. Not only was the hemorrhage arrested, but the tumor decreased to one-quarter its size, and the ulcer began to cicatrize. The patient's strength rapidly increased. Nussbaum, therefore, now proposes completely to prevent the feeding of these tumors by their peripheral nutrient vessels. The blood which supplies the tumor through its base is sufficient to keep it stationary, whilst it does not absolutely starve it. In order to accomplish the desired interruption of the supply from the periphery, Nussbaum urges the formation of a furrow around the tumor, down to the fascia, about one centimetre wide, not with the knife, however, but with the thermocautery. In the above-named case a tubercle had not been included in the ligature. Nine months after the application of the latter, this tubercle had grown, and had thus reached the tumor of the mamma, which tumor had meanwhile become quite solid. The tumor now began to grow again, and even to bleed. Nussbaum, therefore, surrounded all the isolated tubercles with a deep furrow, and their growth evidently stopped. Six weeks later they were still shrinking. Nussbaum thinks that the thermocautery or the actual cautery is much more useful than is generally believed. The subjective condition of the patients, their strength and appetite, are much better after an operation with the cautery than with the knife; the wound heals quicker, except in cases where approximation by sutures can be employed after knife operations; and relapses are much delayed. Nussbaum ascribes these advantages partly to the absence of loss of blood, and partly because the nerves and bloodvessels, being covered with cicatrix, are protected from the influence of the air.—*Physician and Surgeon*, July, 1883.

UTERINE FIBROIDS BECOMING SARCOMATOUS.—At the meeting of the K. K. Gesellschaft der Aerzte, of Vienna, on April 6. 1883, PROF. KUNDRAT presented two specimens of fibroid tumors of the uterus which had become sarcomatous. The first occurred in a woman, æt. 37, who had died of peritonitis from perforating round ulcer of the stomach. The tumor was situated on the posterior wall of the lowest part of the uterus and cervix, was about the size of an ostrich egg, filled the whole pelvis, and projected, by its smaller end, into the enlarged vagina; this portion having the microscopic appearances of a fibroid. Near it, in the posterior wall of the vagina, was a superficial ulcerated nodule as large as a walnut, and numerous metastatic points were found in the liver and both lungs.

The second patient was a woman, æt. 57, a victim to septic poisoning, after an attempt to remove the tumor by laparotomy. The tumor, half as large as a man's head, was situated in the posterior wall of a considerably hypertrophied uterus, with a prolongation between the folds of the left broad ligament. This tumor also contained, as did the first, a softened medullary spot of degeneration, but its original character was clearly shown.—*Medizinische Jahrbücher*, 1883, Hft. ii.

RHABDOMYOMA OF THE ORBIT.—Rhabdomyoma, or a tumor containing striated muscular fibres, is a very rare affection, and DR. S. BAYER, of Stockholm, who describes a case of the kind in a recent number of the *Nordiskt Medicinskt Arkiv*, has met with only twelve instances of it in the course of his reading. His own case was that of a tumor extirpated from a little boy three years old, and which had been observed two months before the operation, penetrating between the globe of the eye and the lower eyelid, and pushing the former from below upward. The tumor was of an oval form, resembling a fibrous tumor in its consistence and in the appearance of the cut surface. On microscopical examination it was found that the neoplasm was chiefly composed of muscular fibres with transverse striæ, but that the greater part of these fibres had at the same time longitudinal striæ. These striæ were either pressed against one another, forming fasciculi arranged in different ways, or diffused in an intermediate substance rich in nuclei. At the points where the fibres are pressed against one another they interpenetrate by points. Caustic potash immediately dissolves the nuclei of the fibres, but causes the distinct appearance of the transverse striæ, while acetic acid causes the nuclei to appear clearly, but dissolves, although slowly, the transverse striæ. A longer treatment with caustic potash or acetic acid causes the dissolution of the whole, as happens in striated muscular fibres, which the striated elements of the tumors resemble, moreover, in this respect—that they present the same double refraction when examined by polarized light. An identical method of examination demonstrated the same property of refraction in the fibres of a tumor of a rhabdomyomatous nature preserved in the Pathological Institute of Stockholm. The mass which combines the muscular elements in the tumor now described by Dr. Bayer, is a delicate granular substance with round nuclei, which, with preparations of osmium, are dissolved in cellules provided with broad or slender prolongations, anastomosing with those of the adjacent cellules. As to the

question of the origin of the tumor, the author can give no other explanation than by admitting the presence of fœtal muscular elements, which probably remained at the base of the orbit, and to the subsequent development of which the neoplasm now described was probably due.—*Med. Times and Gaz.* July 28, 1883.

GELSEMIUM IN SUBMAMMARY INTERCOSTAL NEURALGIA.—J. CHÉRON states that a good many women suffer from a localized pain in the left infra-mammary region, which is due to uterine disease. In some cases it is so painful and persistent that the primary affection may be overlooked. More or less success has attended the use of quinine, aconite, morphine, atropine, the bromides, etc. Chéron has had good results from the use of the following: Tinct. gelsemium gtt. c, simple syrup ʒij, distilled water ad ʒviij. S.—Dose, three to five dessertspoonfuls half an hour before meals, or three hours after. Sometimes there are extremely painful spots in the course of a nerve, and running back to the spine. These may be painted with tincture of iodine every three days.—*Progrès Méd.*, June 16, 1883.

APPLICATION OF IODOFORM TO PERITONEAL WOUNDS.—At the meeting of the Twelfth Congress of the German Surgical Society, KÜSTER explained his method of making iodoform applications to the peritoneum, so as to obtain the full antiseptic action, and at the same time obviate the dangers of toxicity. In cases of ovariotomy, he wipes a sponge, which has been dipped in iodoform, over the coils of intestines, having taken care that all the superfluous powder has been shaken off, so that only a very thin film may adhere to the peritoneum. An iodoform plug is also placed in the vagina. In wounds of the face, about the genital organs, or in other situations where the ordinary antiseptic dressings cannot be easily used, he recommends iodoform collodion.

Among sixty cases treated by him in this manner, there was after-bleeding in only one, all the others having promptly convalesced.—*Deutsche med. Woch.*, No. 17, 1883.

CROTALUS IN MALIGNANT SCARLATINA.—DR. JOHN W. HAYWARD publishes the results of the treatment of two cases of malignant (hemorrhagic) scarlatina by crotalus. In the first case the symptoms were very aggravated on the second day. The throat was nearly closed, and she breathed with difficulty, with an occasional interruption, as though due to the swollen condition of the tonsils and fauces, which appeared softened, jelly-like, and as if gangrenous; the head was thrown upward and backward as far as possible; there was severe retching when anything was put into the mouth, even a teaspoonful of cold water provoked it, and the matter ejected consisted of dirty-brown mucus, apparently colored with disorganized blood oozing from the mucous membrane, or resulting from the gangrenous state of the fauces and tonsils, and there were some bloody streaks as if from blood forced out by the act of retching. After the retching she would fall back on the pillow, moaning in a very weak, low voice, as if dying; and she sank down into the bed in a state of stupid lethargy, like a dying typhus patient. The rash was only faintly visible, and the case apparently hopeless. He determined to try the effect of crotalus, which had been much recommended in the treatment of putrid and hemorrhagic diseases, when introduced subcutaneously as well as by the mouth. The cuticle was consequently removed from round the throat by a cantharides blister, and to the exposed cutis a wet compress sprinkled with crotalus was applied, and renewed at first after half an hour, and then every three hours, also a dose dissolved in a teaspoonful of water was dropped on the tongue every half hour. The effects were watched anxiously. There was no more retching after the first application of the crotalus to the denuded cutis, even when teaspoonfuls of beef-tea were put into the mouth; but the restlessness, pulse, and other symptoms continued much the same all the evening. During the night she gradually grew less distressed and dozed at intervals; toward morning she really slept, and the breathing gradually became less labored and irregular. On the morning of the third day the pulse could be distinctly felt, and was only 120, the respiration was becoming easy, the head was less thrown back, and the struggle appeared to be turning in her favor. The rash was now freely out on the body and legs, but it was of a purple color. The same medicine was continued internally, but now only every two hours, and the crotalus was no longer sprinkled on the compress. During the day the rash gradually brightened in color, and all the distressing symptoms receded rapidly, so that by evening she was able to drink with but little difficulty or pain; the respiration was almost normal; and the pulse was 100, and had gained considerably in force and fulness. From this time improvement went on rapidly, and on the ninth day she was well.

The same treatment was used on the brother of the patient referred to above, and with like happy results. Dr. Hayward has tested the drug again and again, and is thoroughly convinced that crotalus is a reliable remedy in this disease.—*Lancet*, July 14, 1883.

DIAGNOSIS OF URETHRAL DISCHARGES.—P. FÜRBINGER, in a paper in the *Monats. für prakt. Dermatol.*, 1883, Heft 2, enters into a discussion of the differential diagnosis of the different discharges from the male urethra. 1. Spermatorrhœa is the unnatural loss of seminal fluid which regularly occurs during the act of defecation and at the end of micturition (without an orgasm). This condition, more common than is usually supposed, is not the first stage of morbid pollution. Gonorrhœa and sexual abuses are its most common causes. Spermatorrhœa will become azoöspermatorrhœa when the testicles cease to secrete spermatozoa, usually from epididymitis, when azoöspermia exists at the same time. 2. Prostatorrhœa is the discharge of the prostatic secretion, sometimes continuous, at others only during defecation and micturition. It is a rare condition, and a symptom of chronic prostatitis, usually following gonorrhœa. 3. Urethrorrhœa *ex libidine* is a condition usually seen in anæmic and nervous subjects, and consists of a scanty discharge, with strong sexual excitement and erection, but without ejaculation or orgasm. Of these conditions, two are recognized without difficulty. Spermatorrhœa is diagnosticated by the presence of large numbers of spermatozoa—a few signify nothing; and urethrorrhœa by its characteristic mode of onset. The secretion of urethrorrhœa, which comes from Cowper's and the urethral glands, is clear, stringy, and containing only a few epithelial and round cells. Azoöspermatorrhœa is characterized by an abundant discharge, and by the absence of pus-corpuscles, although their presence has no especial weight on the other side, as there may be a coexisting urethritis. In such cases, the diagnosis must be determined by the specific secretion of the seminal vesicles. If a jelly-like substance, resembling grains of sago in shape, be found, it is conclusive; if not found, it may be in solution, and will be precipitated by a concentrated alkali. The secretion is allowed to stand for some time, when a clear drop is placed on a watch-glass, and a drop of caustic potash added. If the drop shows a thick white cloud, or streaks, the secretion contains seminal fluid; if, however, it shows that there

is no azoöspermatorrhœa, the diagnosis rests between prostatorrhœa and chronic gonorrhœa. In gleet the discharge is very scanty, drop by drop. In prostatorrhœa, digital examination of the prostate shows that that gland is painful on pressure.

The indications of prostatorrhœa are: 1. The presence, though not constant, of numerous amyloid bodies. 2. Numerous typical cylinder cells, especially if in the double-layer arrangement of glandular epithelium. 3. The large Böttcher's crystals, the presence of which is ascertained by mixing a drop of the secretion with a drop of a one per cent. solution of phosphorated ammonia on an object-glass. In about an hour, the crystals, numerous, large, and very beautiful, are found. The Böttcher's crystals are a constituent part of the prostatic secretion, and show that the prostate is involved. The prostatic secretion is not, as usually stated, a clear, odorless, thick secretion, but is thin, of a milky cloudiness, and has the characteristic odor of semen. The time at which the secretion appears in the urine is also a diagnostic aid. The first and last portions of the urine must be separated from the principal flow. If the secretion is in the first portion, gleet may be diagnosticated; if in the first and second, prostatorrhœa; and if in the last, azoöspermatorrhœa, which appears in the form of almost transparent, thick, ropy masses, and occasionally in the form of sago grains.—*Centralbl. für klin. Med.*, No. 16, 1883.

CAUSE OF THE HEART-SOUNDS.—DR. C. SANDBORG has succeeded, by means of a special apparatus, in producing an alternate dilatation and compression of the heart of an ox, and consequently imitation of the blood-current and play of the valves. The results show that the heart-sounds, the first as well as the second, are produced by vibrations which originate in the sigmoid valves. The first sound is produced by the vibrations which the blood-current, expelled in systole, produces on the free borders of the valves. The second sound is produced by the tension of the sigmoid valves from the arterial shock, at the beginning of the ventricular diastole. In order to demonstrate theoretically why the tricuspid and mitral valves are so constructed as to make no sound, when in the healthy condition, which is perceptible, Sandborg made the following experiments: 1. He fastened the auriculo-ventricular valves and left the sigmoid valves loose. 2. In another heart the right auriculo-ventricular and left sigmoid valves were drawn back, the mitral and pulmonary valves being left free. 3. In a third heart the aortic and pulmonary valves were fastened, the mitral and tricuspid being free. 4. In a fourth heart, the mitral and pulmonary valves were fastened back, and the tricuspid and aortic left open. These experiments showed plainly that both sounds of the heart could only be heard when the sigmoid valves acted freely, and nothing was heard when the auriculo-ventricular valves alone were free. Sandborg is now studying abnormal heart-sounds, and thus far has been able to reproduce double systolic and diastolic souffles.—*Revue Méd. Franc et Étrang.*, July 7, 1883.

SUPPRESSION OF SALIVA AFTER MUMPS.—MR. A. ST. C. BUXTON reports this case occurring in a lady of middle age. No sense of taste was retained, the mouth felt hot although the temperature was normal; there was intense thirst which was not relieved by drinking, and there was great difficulty in feeding her. Infusion of jaborandi, iodide of mercury, mouth washes of glycerine were equally inefficacious in provoking a flow of saliva. A silver probe was then introduced into the ducts successively, and the negative pole of a thirty cell Leclanché battery applied to the free end of the probe, the positive pole being applied to the nape of the neck. This proceeding produced a copious flow of saliva, which continued in the normal manner. Complete suppression of saliva in both parotids and submaxillaries is a very rare occurrence.—*Lancet*, June 23, 1883.

PREGNANCY AND PARTURITION WITH A CANCEROUS CERVIX UTERI.—PROF. MIDDLETON MICHEL reports the case of a woman, æt. about 30, the mother of several children, who, believing herself pregnant, consulted him with reference to a bloody discharge unattended by pain, which recurred whenever she had sexual intercourse. An examination revealed unmistakable disease of the entire neck, too extensive to permit of its excision, particularly as there were signs of pregnancy, of which she seemed quite sure. A saturated solution of carbolic acid and alum with glycerine was used as an injection awaiting the further development of positive pregnancy. There had been no suspicion of disease, as there had been no pain, and a bloody discharge only after connection. All coition was forbidden during the injections.

At the time of confinement there were no unusual symptoms. She was delivered in due time and without any especial inconvenience of a well-developed child. She herself survived only for three months, in such a state as not to have been able to take care of her infant.—*North Carolina Med. Monthly*, July, 1883.

At the meeting of the K. K. Gesellschaft der Aerzte, of Vienna, on May 11, 1883, DR. FELSENREICH related the history of a case in which pregnancy was complicated by carcinoma. The woman, æt. 30, was the mother of several children. She was admitted to the hospital at the end of the eighth lunar month. During the sixth month there was a period of five days' hemorrhage, but there was no other hemorrhage. On February 28th, there was an escape of liquor amnii, labor set in on March 1st, and on March 4th she was brought to the clinic (in the eighth month). The carcinoma had then extended over the infra-vaginal and middle part, and digital examination showed a stiff and friable condition of the cervical canal. Spontaneous delivery being regarded as impossible, with the consent of Dr. Braun-Ferwald, Dr. Felsenreich removed the carcinomatous growth, and thus made room for the delivery. A female child was delivered, and the placenta soon followed spontaneously. The uterus was then thoroughly disinfected and removed by the supra-vaginal operation, and the wound dressed with iodoform. On the third day urine escaped per vaginam. A carcinomatous nodule was found to be the cause of this, was removed, and the fistula closed. Two months afterward the woman was, to all appearances, well.—*Medizinische Jahrbücher*, 1883, Hft. ii.

CONSOLIDATION OF FRACTURES IN DIABETICS.—During the past eleven years, M. VERNEUIL has made four personal observations on this subject. The first case was one of compound fracture of the left leg; the second a fracture of the humerus in a badly nourished patient; the third a fracture of both bones of one forearm; and the fourth a fracture of the lower extremity of the radius. These patients were all diabetics. Though this is too small a number of cases from which to draw general conclusions, they tend to show the evil influence of diabetes on the formation of callus. In only one case of the four did the formation appear to be normal, and in the other three the formation was very defective. By a singular chance, these three cases represented the three different degrees of the same lesion—tardy consolidation, imperfect consolidation, and non-consolidation. In investigating the causes of these anomalies of the reparative forces, it is difficult

to dispute the part that the dyscrasia must play, for in each of the three cases the fracture was easily reduced and kept in place by suitable apparatus. There was no fault to be found with the mechanical part of the treatment. No difficulty was experienced in keeping the small wound closed in the compound fracture of the leg, and there was no suppuration.

M. Verneuil concludes his paper with the following propositions: 1. The retardation and absence of consolidation found in the three cases seem to be due to diabetic dyscrasia. 2. This retardation and absence of consolidation necessarily imply a diminution or suppression of the reparative forces, and particularly of nutrition. 3. From these facts, we may conclude that diabetes, when it embarrasses or stops the formation of callus, does so, at least indirectly, by impairing nutrition.—*Gaz. Hebdom.*, July 27, 1883.

INTRAVENOUS INJECTION OF SALINE SOLUTIONS.—DR. L. SZUMAN reports a successful case of intravenous injection for anæmia after traumatism and operation. Besides this case, he mentions eight successes, and gives the following as the advantages of this therapeutic measure: 1. One can always get the requisite fluid. 2. The operation requires no special apparatus, as an injection apparatus and a canula or probe-trocar are always at hand. Thorough antiseptic precaution must be observed with regard to the instruments. 3. Saline solutions in cases of acute anæmia must supersede transfusion of blood.

Dr. Szuman's case was that of a boy, æt. 15, who had sustained a compound fracture of the surgical neck of the right humerus, with laceration of the capsular ligament, and fractures of the right femur and tibia. When seen, the patient was in a state of collapse, the face, lips, and conjunctiva bloodless, and he was almost pulseless. Szuman decided to resect the shoulder-joint, and chloroform was cautiously administered. The wound was dressed antiseptically. The collapse was so great, however, that artificial respiration was employed, the head lowered, and ether injected hypodermatically, with the result of rallying the patient. On the next day, the lips were blanched, he was very feeble, and rejected food. Vomiting set in in forty-eight hours. During a change in the dressing of the wound, convulsive twitchings came on, increased, and the patient became unconscious. As the patient seemed to be dying of cerebral anæmia, ether was again injected, the upper extremity lowered, the limbs rubbed with camphorated spirit, and the uninjured limbs bound with elastic bandages. Intravenous injection of saline solution was then made: ℥iss of common salt and grs. xv of carbonate of soda were dissolved in ℥xxxivss of distilled water. A carbolic disinfectant was employed, there being no transfusion apparatus at hand; a small drainage-tube was fixed to a fine trocar; this was inserted into the median vein (into which the canula was fixed), and the fluid injected until about ℥viij had been injected, when the patient opened his eyes, the spasms stopped, and he began to talk. After about ℥xxiv had entered the circulation, the pulse fell from 144 to 112, and he began to complain of being cold; the injection was accordingly discontinued, and the ligature removed from the vein. From this time, he steadily improved, and finally completely recovered.—*Berliner klin. Woch.*, No. 21, 1883.

TREATMENT OF PHIMOSIS BY DILATATION.—M. DE SAINT-GERMAIN considers dilatation as the best means of treating congenital phimosis, even in the adult. He uses Nélaton's dilator with three blades, but has successfully used Trousseau's double-bladed dilator. He thinks that dilatation is applicable to all cases of phimosis, except those in which the preputial mucous membrane is thickened and of almost cartilaginous hardness. The blades of the dilator are introduced as far as the base of the glans penis, the instrument opened, and the amount of the constriction produced on the mucous membrane of the prepuce is determined by the degree to which the instrument opens. When the double-bladed instrument is used, the dilatation converts the narrowed preputial orifice into a buttonhole slit, and a second dilatation completes the operation. It then only remains to break up the adhesions existing between the gland and the prepuce, and during the next few days the prepuce should be regularly drawn back, and the parts dressed with borated vaseline.—*Revue Méd. Franç. et Étrang*, June 9, 1883.

PATHOLOGICAL CHANGES IN COLCHICIN POISONING.—At the close of a paper on the pharmacological study of colchicin, DR. HEINRICH PASCHKIS thus summarizes the anatomical changes found after death: The lungs are, as a rule, pale, and contain but little blood. In only one case did the lungs contain much blood, and several times there were hemorrhagic spots on the pleuræ, absence of diarrhœa, with vomiting and cramps. The heart is seldom empty of blood; usually the right or both ventricles were filled with black thrombi. In one case, the pericardium contained a hemorrhagic spot. The peritoneum is generally injected, with small hemorrhagic foci here and there, the same condition being true of the diaphragm. The omentum also contains hemorrhagic spots. The spleen is generally contracted, wrinkled, bloodless, and the liver containing but little or no blood. The kidneys are pale; the cortical and medullary substance bloodless. The bladder is moderately contracted, containing a little urine. The mucous membrane of the stomach and intestines is reddened and markedly hyperæmic, with numerous hemorrhagic spots. Microscopic examination shows widespread extravasation in the submucous tissue and partial lifting up of the epithelium in the mucous tissue by small extravasations. These hemorrhagic spots are found in the whole of the intestinal canal and in the stomach, the folds of the mucous membrane being always prominent; they are frequent and large, except at the pylorus, which is always firmly contracted, and in the region of the ileum and large intestine; they are punctiform, striated, or streaked often to such an extent that the mucous membrane resembles coarse bluish-red cloth. The contents of the stomach are a clear to dark-yellow mucus, sometimes containing transuded blood. Finally, the membranes of the brain are hyperæmic.—*Medizin. Jahrbücher*, 1883, Hft. ii.

MICROBES IN THE LIQUIDS OF THE NORMAL ORGANISM.—MM. OLLIVIER and RICHET have recently made further investigations on the existence of microbial elements in the organic liquids of fish in the normal state. They have already found microbes in the lymph of the peritoneal, pericardial, and cerebro-spinal cavities. Now they announce the discovery of these elements in the blood of fish, taken during life, and from the heart itself, with proper antiseptic precautions. Culture of these microbes has not caused putrefaction in clean, hermetically closed tubes, while the blood, placed in open vessels, has undergone rapid putrefaction. They conclude that the lymph and blood of fish normally contain microbial elements, of which the nature and development remain to be determined.—*Gaz. Hebdom.*, July 27, 1883.

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SATURDAY, AUGUST 18, 1883.

CHOLERA PREDICTIONS.

THE panic caused by the rapid spread of cholera in Egypt is subsiding in the absence of cases in European ports, and the representatives of commercial interests are now inclined to think with Noah's neighbors of old, that "it isn't going to be much of a shower after all." Among others the United States Consul at Smyrna, Mr. W. E. Stevens, in his report of July 13th, adopts the theory, at first offered by British officials, that the Egyptian outbreak is not true Asiatic cholera, but a disease of local origin, due to bad sanitary condition and impure water supply. It is needless to occupy space with the arguments which he gives in favor of this theory, for by this time those who advanced it are probably satisfied that if the disease is not Asiatic cholera, it is not distinguishable from it as to symptoms, results, or power of infection. The chief argument in favor of its local origin is one that is always urged by a certain class in such outbreaks, viz., that there is no evidence of its importation.

Negative proof of this sort is proverbially weak and unreliable, and the fact that the Commission which visited Damietta in the midst of the panic of the first few days after the announcement of the presence of the disease, found, during the two days of its stay, no evidence of importation, is almost without significance. It would have been strange indeed, under the circumstances, had they found such evidence. At present the flame of the epidemic is burning lower, in some places for want of susceptible material, in others because of isolation and disinfection of the first cases, or because of some peculiarity in the local and atmospheric conditions.

To conclude from this state of things, however, that the danger is less, or has disappeared, is foolish. We have previously stated that it is improbable that cholera will reach the United States this year, but that it is an even chance that we shall have it next year. Since that opinion was published the danger has diminished very little, if any, and while there is not the slightest occasion for panic, there is very urgent need of enforcing cleanliness, of looking to the purity of water supply, and of great care in the administration of our quarantine stations.

HAY FEVER.

ALTHOUGH our title is a misnomer, the disease is a reality, and one which calls for careful study in view of the apparently ever-increasing frequency of cases of the disorder. Some years since, Dr. Wyman, of Boston, who has published by far the best book upon the subject, essayed a more definite and scientific appellative, and, were it not for the fact that very many cases appear in the early summer, "autumnal catarrh" would be the most appropriate title. But, as a matter of fact, many cases occur in the month of June, and some few have been observed by the late Dr. Beard in July, so that, to include all cases, the title of "Periodical Catarrh" would be perhaps best, and certainly most accurate. It may be, however, that, as in many other affections, usage will continue to apply the title whose only claim lies in antiquity, and that as many of us insist upon calling enteric fever typhoid, we shall continue to speak of hay fever, because it occurs at the time of hay harvest in England, and prevails at the same time, but long after that harvest is ended in America.

One thing is, however, certain, that there is an increasing number of persons who at one stated season of the year, somewhere between June 1st and October 1st, suffer from catarrhal attacks of greater or less severity, from which they are generally free at other seasons. In a large majority of cases the catarrhal symptoms are confined to the nasal and conjunctival mucous membrane, but in a considerable number there is a development of bronchitis with more or less well-marked asthmatic symptoms. Starting with the conjunctiva, and extending to the minute bronchial tubes, all the intervening mucous membrane is liable to attack, and the gravity of the symptoms of the disorder are in pretty exact accordance with the amount of mucous membrane involved.

But our object is not to write a treatise upon an affection notoriously unamenable to medical treatment, but merely to comment upon one or two

curious, and as yet not satisfactorily explained, peculiarities connected with it.

One of these is the very evident increase in the number of cases. Further than the fact that, owing to the efforts of the United States Hay-fever Association, the disease escapes classification as an ordinary cold, and is recognized by the single element of its periodicity, there does not seem to be any sufficient reason for this increase. For while the affection attacks principally those well-to-do in life, there are numerous exceptions to the rule; and while many of those who suffer from it belong to the category of nervous individuals, very many others would never be classed as such, and but few give evidence of being the subjects of suppressed gout.

Quite recently, Dr. Harrison Allen and others have noticed a peculiarity in the anatomical position and shape of the inferior turbinated bone, which, to say the least, is frequently associated with autumnal catarrh; but such a peculiarity can in no way account for the periodical character of the affection.

So far as the present state of our knowledge extends, we are compelled to fall back upon that most unsatisfactory and shadowy cause—idiosyncrasy—acted upon by the inferred influence of vegetable matter. Attempts have been made by Blackley, Marsh, and others to fix upon certain particular pollens, as those of the grasses, maize, and ragweed, as the especially exciting cause; but it would rather seem that many varieties of vegetable dust are equally efficient. We are not aware of any observations which have connected the affection with bacteria; nor can we reasonably hope for any solution of the problem which neglects the *seasonable* existence of the exciting cause, although it can be readily seen that the idiosyncratic susceptibility to that cause may be anatomical.

While remedial measures can do much to alleviate the severity of the disorder, it is becoming pretty well understood that a cure can only be obtained by a flight to one of the localities which experience shows to enjoy exemption from the presence of the exciting cause. These localities are quite numerous, and are being annually resorted to by large numbers. The Hay-fever Association has published a large list of such places, and has placed its estimate upon their respective value, but these estimates are principally based upon lay opinion, and are subject to some qualification on the score of inaccurate observation. There seems to be no doubt that at some high elevations, and some low ones, relief is obtained. The essential point would seem to be absence of much upland vegetation. Thus it is that some portions of the White Mountains, and certain seashore resorts situated upon sandy islands entirely surrounded by water, as Fire Island, New York, and Beach Haven, New Jersey, have acquired their reputations, while many others upon the Association's list are probably of equal value. There is little or no doubt that upon the ocean itself the exemption is most perfect. Whichever locality is selected, it is important that the patient should go there early, if possible, before the beginning of the attack, and that a quiet and prudential life should be led.

Observation would seem to show that those who suffer most at home are best satisfied with the measure of relief obtained, although the actual exemption is more perfect in those who merely suffer from an annoying coryza and irritating cough at home. Experience would also seem to show that in some cases, at least, the regular and systematic resort to one of the favorable localities is attended with increasing benefit, although there appears to be no evidence that the tendency will ever entirely die out in adults. In children, it would seem to be most important that every effort should be made to keep the disorder in abeyance, by sending them early and regularly to a suitable location, in the hope that the habit of system may not become ineradicable, but may disappear before the advent of adult life.

THE COLLECTIVE INVESTIGATION OF DISEASE IN ENGLAND AND AMERICA.

THE idea of collecting, combining, and analyzing the experience of a large number of those physicians who are engaged in general practice, is not by any means a new one; but until quite recently no definite, well organized attempt has been made to carry it into effect.

Individual efforts have been made here and there to obtain such information by means of circulars containing lists of questions, and when the querist has been a man well known to, and respected by the profession, some interesting results have been obtained in this way.

The organization of the Collective Investigation Committee of the British Medical Association marks a step in advance in this direction, and now that this Committee has proposed to the American Medical Association to join in the work, some account of the organization and methods of the English Committee may be of interest.

The matter was under discussion in the British Association for two years before the present organization was completed and work fairly commenced. The Collective Investigation Committee is a Committee of the general Association, and is charged with the duty of selecting the subjects to be investigated, and of preparing the questions to be sent out. The formulating these questions is one of the great

difficulties in the work; it can only be done successfully by those who are fully conversant with all that is known with regard to the subject under consideration, and who can indicate briefly and clearly those points which are still doubtful or unknown, but which it is fairly probable may be settled if the attention of a large number of practitioners be called to them, and the results of their observations are collected, compared, and properly analyzed.

The method adopted by the Committee is to prepare, or have prepared by experts, a memorandum on the subject of inquiry, giving as briefly as possible the state of existing knowledge with regard to it, and to issue with this a card containing the questions to be answered.

For example, the memorandum on acute pneumonia, explains that the special point to be investigated is that of its etiology, and more particularly as to what grounds there are for supposing that it may sometimes, or often, or usually, be due to infection or to some more or less specific septic influence. It then goes on to explain the purpose of the individual questions, and the points to be kept in view in answering them. The card issued with this memorandum is accompanied with a specimen of a "filled up" card. It asks for the initials of the patient, the sex, age, occupation, conjugal condition, whether temperate, food; residence, whether high or low, etc.; atmospheric conditions; number of cases of the disease in the same house—in the same district; sanitary conditions of house and district; family history of lung disease, etc.

The memoranda and cards thus far issued relate to diphtheria, chorea, rheumatism, pneumonia, and syphilis, and while of unequal merit are nevertheless all good, and well calculated to produce good results.

Turning now to the machinery for placing these cards in proper hands, we find that this is done to a great extent by local committees of the various local, or, as they are called in England, "Branch" Associations.

The appeal of the general Committee for the formation of these local Committees sets forth the importance of the work, pointing out that a large part of our present knowledge of disease is derived from hospital practice in which little or no opportunity occurs for the observation of the conditions which precede established organic disease. It explains also the failure of previous efforts as due to the circulation of questions requiring complex and elaborate answers, usually relating to past and unrecorded experiences, and to the want of general interest in the subject.

At the recent meeting of the American Medical Association in Cleveland, Dr. Billings presented communications received from Dr. F. A. Mahomed, the Secretary of the General Committee of the British Association, inviting coöperation on the part of American physicians, and expressing a hope that this coöperation might be furnished in an organized and systematic way through a Committee of our own Association. The papers were referred to the Standing Committee of the American Medical Association, which is charged with the investigation of the relations of disease to climatic conditions, of which Committee Dr. N. S. Davis is chairman, and they will, no doubt, receive due consideration.

The advantages of extending the field of observation and inquiry over this country are sufficiently obvious, and it is a happy thought to provide objects of a common interest for the two Associations which will tend to bring the physicians of the two great English-speaking nations of the earth into closer relations with each other. The problems of English physicians are, to a great extent, our own problems also, and we can, no doubt, both help and be helped in this matter.

The English Committee already find the material which they have collected so abundant that they propose to publish it in the form of a semi-annual record. Perhaps this record may become a truly international one. No doubt there are some rather special difficulties in the way, but we think all these can be overcome by tact and perseverance. After all it is but the extension of the principle of association, and division of labor, which has in other fields produced such excellent results, and we hope that the Committee of the American Medical Association which now has the matter in charge, will be able to agree upon some plan which will be a practical acceptance of the invitation of the British Committee.

In the words of Sir William Gull, "This is the age of combinations, but there is none which has a better objeet than this; for what can surpass that whose purpose is the investigation of truth for the good of man?"

"MY ASYLUM LIFE."

OUR attention has been attracted by an article in *Lippincott's Magazine* for July, 1883, entitled "My Asylum Life, by a physician." It purports to be an autobiographical account of a case of insanity, with criticisms on asylum management. Either it is fiction, written by a practised hand, in order thus artfully to attack the system pursued in asylums, or it is a true story. In either case, it is of interest to the physician, since even if it be untrue as a personal narrative, it exhibits a curious knowledge of the routine of life in hospitals for the insane, and would be, in any point of view, worth considering by those who have charge of them. On the whole, after carefully examining the evidence afforded by the narrative, we are inclined to believe

it fictitious, although as to this there is and will be some difference of opinion.

The case is described as a rare example of double self-consciousness, involving, finally, when at the worst, a belief in the fact that the patient was himself and also Satan. As a statement of his own case by a recovered patient, the continuity of the psychological history and the accuracy with which, after years have passed, conversations are related, make rather too heavy a tax on our faith. At the same time, the analytic study of the case in its symptoms and the story of the rise and progress of a despotic belief in a delusion incline us to believe that in some sense the account is true, and that whoever wrote it must be much in contact with the insane.

In the patient's distress, he consults at least two physicians, and, of course, as a doctor, comments sharply on their views and manner. It is at the close of one of these interviews that his "Satan-self" threatens to infest him with other devils, on which the patient, in a moment of clearness, reflects that, although double personality is well known, for a man to regard himself as more than two persons is an unknown case, and, therefore, not to be feared. We should like to know if this be so.

The idea that "Sleep is a widely illustrated insane asylum" is scarcely new, and is, perhaps, rather too fanciful. But however curious may be the psychological study with its points of harmony and grim description of "pure terror" as a possible mental condition—a part of the case quite too dramatic in its presentation—the practical value of the article must lie if anywhere in its hints as to the evils and difficulties of asylum life.

The sharp comment on the failure of intelligent study and tactful treatment of cases during the first day or two after admission struck us, as well as the complaint as to the general incredulity in regard to the statements of those once insane, a point, we fear, but too well taken. We were impressed, too, by the remarks on the fact that the asylum doctor habitually believes asylums the place for all insane people, and that, in proof of this, we see no statements in their returns of releases within a day or two after admission of those whom they feel were best treated outside of stone walls.

The further comments on the diet, on the want of trained male nurses, on the insufficiency of medical care, the neglect of thephysical ailments of the lunatic, on the horrors of the lock and bar system, and the reasons for the frequent failure of asylums to secure the highest medical skill, are all alike the more valuable because of a certain charitable calmness with which the charges are made, while the pitiless sarcasm on the managers of asylums, and on the prevalent methods of inspection, might lead us back to the belief entertained by many in regard to this paper that it must have been written by one whose statements were emphasized by personal suffering.

But whether a sad and true story or a clever assault on asylum management in the guise of a fictitious sketch, it cannot fail to suggest how great would be the interest of a volume of autobiographical statements of cases of insanity, nor to awaken afresh the question as to whether the general medical opinion is correct in its belief that there is still much to amend in our care of the insane.

THE RIGHT OF AN AUTHOR'S OWNERSHIP IN HIS LECTURES TO BE VINDICATED.

THREE weeks ago we published letters from Drs. Alfred Stillé and Hayes Agnew, denouncing as fraudulent *An Epitome of Medicine and Surgery*, published in this city by Samuel M. Miller, M.D., and purporting to be by them. We now learn that, upon their affidavits that the publication is unauthorized by them, the Court has just granted a preliminary injunction restraining the circulation and sale of this book. At the hearing, it is expected that the Court will give equitable relief to Drs. Stillé and Agnew by granting a perpetual injunction and damages.

Authors have been imposed upon so long by the unauthorized and even fraudulent publication of their lectures, that they finally began to believe that they had no right of property in them which piratical publishers were bound to respect, and these publishers in turn, in the absence of protests, gradually began to think that this apparent view must be practically correct. In the present case flagrant injustice has been done Drs. Stillé and Agnew, whose lectures have been stolen, for they are made to masquerade before the profession as authors of a treatise which they never wrote, and which they indignantly repudiate, and the case is particularly aggravated by the fact that Dr. Miller took out a copyright on the book without apparently the slightest shadow of authority.

We are very glad to learn that the common-law right of an author's ownership in his lectures is soon to be again vindicated in our courts, and we trust that this will stimulate our clinical lecturers to stop the publication of unauthorized reports of their lectures, which, by reason of the frequent errors and gross inaccuracies, have proved as great a fraud upon the reader as they were upon the rights and reputation of the authors.

In this connection, it affords us no little satisfaction to state that THE MEDICAL NEWS has always acted in accordance with the view, which, over and again, has received judicial sanction, and that the lectures which have appeared in our

columns have always been authorized publications, made by or with the express permission of their authors, and their right of ownership protected as well as acknowledged by the tender of pecuniary compensation.

PHENIC CAMPHOR IN THE TREATMENT OF ATAXIC TYPHOID.

UNDER this title, a French army surgeon, DR. AMAT, addresses a note to the editor of the *Bulletin Général de Thérapeutique* of a recent issue. His object is to inform the medical profession of the good results obtained from the administration of phenic camphor in low cases of typhoid fever. What is phenic camphor? When carbolic acid and camphor are rubbed up together with alcohol and water, a mixture results. The exact proportions recommended by Dr. Amat are the following: Camphor, 5 grammes (about 80 grains); crystallized carbolic acid, 2½ grammes (about 38 grains); alcohol, 150 grammes (about 5 ozs.); and water, 850 grammes (about 27¼ ozs.). This prescription is used by the stomach and rectum, and is reserved for the cases characterized by great depression, with nervous excitement. It is presented to our readers with the view that such a combination may offer some advantages in the class of cases above referred to.

SOCIETY PROCEEDINGS.

BRITISH MEDICAL ASSOCIATION.

Thirty-first Annual Meeting, held at Liverpool, July 31 and August 1 and 2, 1883.

TUESDAY, JULY 31. MORNING SESSION.

THE fifty-first annual meeting of the British Medical Association was held at Liverpool on July 31st, August 1st, 2d, and 3d, under the presidency of Dr. A. T. H. Waters, of Liverpool.

Mr. Wheelhouse presented the

REPORT OF COUNCIL,

from which it appeared that the total number of members on the register on July 1st was 10,150. The revenue of the Association for the year was £19,076, and the expenditure and loss were £16,288, leaving a surplus of £2,788. The invested funds, exclusive of trust funds, amount to £13,267.

The Association then proceeded to the consideration of new by-laws, after which it adjourned.

EVENING SESSION.

The members reassembled in the evening, when the President announced that the meeting was honored by the presence of several distinguished foreigners, amongst whom were Drs. Austin Flint, F. S. Dennis, and George M. Lefferts, of New York; Prof. Snellen, of Utrecht, and Prof. Tilanus, of Amsterdam.

The PRESIDENT, DR. A. T. H. WATERS, then delivered his

ANNUAL ADDRESS,

choosing for his subject the "Present Aspect and Future Prospects of Medicine."

He said: In considering the standpoint of medicine, we should always bear in mind that medicine must be judged by the therapeutic results which it achieves, and in proportion as we can show that the researches of the physiologist, the pathologist, and the chemist, and the application of scientific principles, have enabled the physician to deal more successfully with disease, to anticipate its development, to recognize its earliest symptoms, to mitigate its severity, and reduce its rate of mortality, so do we establish for our art a claim on the confidence and gratitude of mankind, and in this respect the present state of medicine shows no slight advance on the past, and promises a more rapid progress in the future. The past five and twenty or thirty years—not to go further back—have been marked by the general development of views with reference to many diseases, such as have largely and beneficially influenced our treatment of them. With regard to the affection which we term pneumonia, have we not seen the introduction into practice of a mode of treatment which has deprived the disease of much of its terror, and greatly reduces its mortality? Whatever be the views which we entertain of the nature of pneumonia, the important fact remains that we may enumerate amongst the successes of medicine of the present day, a large reduction in the mortality from the affection.

Again, the recent development of the practice of tapping the chest in pleuritic effusion—an operation comparatively rarely performed thirty years ago—is, in my opinion, a very marked improvement in therapeutics. Those who have carefully watched the progress of medicine in this matter, know full well how great have been the changes which have taken place within the last quarter of a century, and especially since the introduction of the aspirator. It is impossible to refer to all those to whom we are indebted with reference to this subject, but Trousseau as a pioneer, and Bowditch, whose large experience and satisfactory results exercised so beneficial an influence, I must not pass over, whilst there are many in this country to whom we owe no less a debt of gratitude. There is, perhaps, from the facility with which tapping may be performed, and the almost entire absence of risk when it is properly performed, a fear that it may sometimes be resorted to unnecessarily. But, from what I may term a somewhat large experience in the operation, I can speak with confidence of its value and safety in both acute and chronic effusions, and I cannot but think that, as the result of the early performance of the operation, we shall see, every year, fewer of those cases of deformed chest which so frequently follow neglected pleuritic effusion.

It is impossible to enumerate the various affections in which there is a marked improvement in treatment, and those which I have referred to must be considered simply as illustrations of my position; but whilst I speak with satisfaction on this subject, I must not forget to mention that, in the midst of many successes, we have instances of, I will not say actual, but comparative, failure. Our knowledge of the pathology of some affections remains very imperfect, and our therapeutics are in some respects very unsatisfactory. Although rheumatic fever is one of the most common diseases, it must be confessed that we are ignorant of its pathology. During the past few years there has been introduced into practice a mode of treating the disease by the compounds of salicylic acid, which has met with general adoption, and has been largely successful. I can add the testimony of my experience in its favor; but still, with all the opportunities which we possess of studying the affection, we are as yet unable to give any satisfactory explanation of its essential nature. But, gentlemen, of all the questions which now engage the attention of the pathologist and physician, there is no one which surpasses in importance

that which relates to the dependence of certain diseases on micro-organisms. The subject is one of the greatest possible interest, and fraught with bearings of a practical character which already have had, and are doubtless destined still further to have, a vast influence on the prevention and treatment of disease. The establishment of the fact that pebrine, chicken-cholera, and splenic fever depend on the presence of specific microbes, and that relapsing fever has associated with it, if not indeed causing it, a like organism must be classed amongst the most striking discoveries of the present day, and undoubtedly mark an epoch in the history of pathological science; whilst the gradually accumulating evidence that tuberculosis is the result of a parasitic element seems likely to lead to its being placed in the same category as those affections the pathology of which appears now to be clearly established. Moreover, the brilliant results which have followed the process of attenuative culture of the virus of some of the affections to which I have referred, and the protective influence of vaccination by these attenuated fluids, as carried out by Pasteur, lead us to hope that, by further researches in the same direction, we may yet discover some means for checking the ravages of tuberculosis and other allied diseases, as certainly as the great discovery of Jenner has enabled us to check the ravages of smallpox.

Of the many problems which await solution in connection with the subject which I have just considered, no one is more pressing than that of the dependence, or otherwise, of pulmonary consumption on the bacillus which has been so largely found, not only in the morbid deposits which result from the disease, but in the expectoration of phthisical patients. Should it be found that in all cases of genuine tubercular phthisis the bacillus tuberculosis is present, and should future researches show that the disease is caused by the parasite, a solidity will be given to the pathology of the affection, and perhaps the therapeutics of it will be materially aided. Speaking from clinical experience, I cannot but conclude that the disease to which we give the name of phthisis has more than one mode of origin. Further investigation will perhaps clear up the doubt which exists on this point; and here I should like to observe that there are some affections of the lungs which closely resemble, in their clinical features, acute phthisis, and yet which, under careful treatment, do not go on to a fatal issue. Of such cases I have now seen several. Their general symptoms, their physical signs, their temperature-ranges, made me conclude that I had to deal with cases of acute pulmonary tuberculosis, and I have expected a fatal result, but recovery has taken place. These cases occurred before attention was directed to the presence of bacilli in the sputum of phthisical patients, and no examination of this secretion was made. In instances of a similar kind, we shall now be able to use this method of investigation, and possibly it will afford a valuable means of differential diagnosis, and enable us to give, in cases where the bacilli are absent, a more favorable prognosis than the general symptoms would otherwise warrant. I may say, in regard to the cases to which I have referred, that they were treated by free nutrition. the administration of quinine, and a somewhat liberal quantity of alcoholic stimulants.

The great point which remains to be decided, and which I hope the discussions which will take place at this meeting will help to decide, is whether the so-called bacillus tuberculosis is the cause or the consequence of the tuberculous disease.

WEDNESDAY, AUGUST IST.

The President of Council stated that a cordial invitation had been received to hold the next meeting of the Association in Belfast, and he moved that the invitation be accepted, and that Dr. Cuming of Belfast be appointed President-elect. Unanimously adopted.

MR. REGINALD HARRISON then delivered the

ADDRESS IN SURGERY.

After expressing his high appreciation of the honor conferred on him, he referred to the surgical traditions of Liverpool, and then dwelt upon some of the more recent advances and work in connection with the surgery of the urinary organs.

He said, commencing with the kidneys, we are at once struck with what surgery is doing for them. Until quite recently the diseases of these organs were regarded as belonging almost exclusively to the province of the physician, and probably they would have remained so had preventive medicine obtained fuller development.

A more extended acquaintance with the pathology of the kidney has brought to light conditions in which the work of the physician requires to be supplemented by that of the surgeon. Pain arising from an undue mobility of the organ, tumors, deposits, hemorrhages, and collections of fluid within it—all these morbid states are now recognized as capable of relief or cure by fixing, opening, or extirpating the abnormal or disordered organ; whilst numerous illustrations have already been afforded of the successful removal of stones from positions where they must have ultimately led to the disorganization of the kidney in which they had become impacted.

It would be premature at present to endeavor to formulate anything like precise rules in reference to the application of the various operations on the kidney to which I have briefly referred; they are at present comparatively new to us, and we must occupy ourselves in cautiously moving in the direction they indicate, and in collecting experience rather than in drawing conclusions other than very general ones.

Amongst many valuable contributions to the literature of this subject which have appeared, I would include one by Dr. R. P. Harris, of Philadelphia, which contains an analysis of one hundred cases of nephrectomy. From this, as well as other communications which have more recently been published in this country, we may draw at least three conclusions of value, so far as the operation of extirpation of the kidney is concerned. These are:

1. That nephrectomy has been the means of saving many lives under circumstances where no other method of treatment was likely to be of service;

2. That this operation has been practised in cases where the probability of a successful termination appeared to be very remote; and

3. That a method of effecting the removal of the organ different from that which was selected, or a procedure less heroic, might, in some instances, have tended to increase the chances of success.

In these directions then—in selecting cases for operation, in rejecting others as unsuitable, and in determining relatively to the case in question the best method of procedure—I take it that good work has yet to be done.

It appears to me that among the difficulties we have to contend with in the application of nephrectomy, two stand out prominently: (1) The kidney it is proposed to remove may be the only one; (2) the opposite organ may be similarly affected, though in a less degree, yet sufficient to interfere with those compensatory changes being carried out which are essential when one excretory organ has to supply a lack of service on the part of another. The literature of this subject will already be found to illustrate the class of difficulties to which I am referring.

Till a few years since, the removal of stone from the bladder by crushing had been conducted on the lines laid down by Civiale some half-century ago. Though this method of proceeding has included amongst its advocates, past and present, surgeons of eminence, it cannot be said that, as then practised, it was either gaining ground or confidence. Indeed, I think I may say there was an increasing tendency to limit its application, and to substitute lithotomy in all cases but those of the simplest kind. The mortality consequent upon retention of broken calculi within the bladder was sufficient to induce all but the most ardent admirers of lithotrity to hesitate to give it the preference over a proceeding in which, whatever might be the risks, there was at least a guarantee that the whole of the stone had been removed.

Whilst the surgical mind was thus to some extent in doubt as to the limits to which the crushing operation of stone might safely be pushed, two important communications followed rapidly upon each other. That both of them should have emanated from America merely indicates that the desire to advance the art of surgery is not limited to the old country, but is a natural outcome of advancing civilization and humanity.

The first of these papers was by Dr. Otis, of New York, who demonstrated beyond all reasonable doubt, and in a manner which had not previously been attempted, that the male urethra was capable of safely receiving far larger instruments than were generally employed. Following upon this, and probably influencing the views of the author, came Dr. Bigelow's paper on the removal of stone from the bladder by crushing, and withdrawing it at a single operation; the latter communication clearly showing that the bladder was tolerant of much more prolonged manipulation than had previously been believed.

It appears to me that the originality of Bigelow in no way detracted from the importance of the work that had previously been done in this country and elsewhere, or compromised the acumen of those who were most interested in the progress of this department of surgery.

Passing to tumors of the bladder, it is not surprising, seeing what has been done for tumors of the ovaries, uterus, and intestines, by Sir Spencer Wells, Keith, Lawson Tait, Treves, and others, that growths occupying the interior of the bladder should have received special attention. Though the literature relating to this subject has been of a somewhat fragmentary character, such compilations as Stein's recent work, *A Study of Tumors of the Bladder*, conclusively show that some gratifying results have already been attained in both sexes. The great distress connected with this class of growths, the uncertainty as to the precise nature of the affection, in the first instance, and subsequently the kind of symptoms that accompany it have naturally suggested the employment of means having for their object their removal by operation.

Sir Henry Thompson has done good service in giving prominence to the employment of digital exploration of the bladder, and in furnishing illustrations of the great advantage that this proceeding is capable of affording in suitable cases.

From a consideration of some of the extremely valuable records which have been published by various surgeons, where the bladder has been opened for the removal of tumors, it appears to me that it might have been better had the operative proceeding terminated with the detection and exploration of the growth by the finger. The chief dangers which experience has shown to be liable to attend the performance of this class of operations are:

1. The chance of rupturing a bladder, the coats of which have been rendered less resisting than natural.
2. The provocation of a hemorrhage which it has been found difficult to control.
3. An incomplete removal of the growth.

On the other hand, an examination of many tumors of this kind, of which villous growths or tufts furnish the best examples, shows that there is nothing in their connections or relations which need necessarily interfere with their complete removal. Precise information as to the presence and attachments of these growths we may now obtain with comparative safety. Whether their removal is to follow upon their discovery will be matter for further consideration.

It appears to me that enlargement of the prostate is specially interesting to us in relation to its earliest and to its most advanced forms, and it is to these points that I shall more directly refer.

I have long been led to believe that our treatment of prostatic stricture—or by whatever name we know it—commences, as a rule, far too late; we delay until the bladder shows, by the formation of a pouch, or a saccule, behind the prostate, the first bad influence of back-pressure before we seek to rectify it.

I have endeavored to prove how much good may be done by the adoption of judicious mechanical treatment on the appearance of indications that the prostate is commencing to obstruct micturition, and I have founded my suggestion upon a condition which may be seen illustrated in any museum, viz., one in which, though the gland has become large, obstruction has not been known to occur. An extended adoption of this practice has convinced me that the pressing symptoms connected with an enlarging prostate may be kept in abeyance by the timely employment of those principles of treatment which are generally recognized as being applicable to any tubes within the body which are threatened with occlusion, and are within our reach.

In the more advanced forms of prostatic enlargement, where the bladder has been converted into a receptacle little better than a chronic abscess in which urine stagnates, surgery has done much to afford relief.

When the comfort that catheterism is capable of affording has ceased to be effectual, other plans of establishing a drain for the urine are at our disposal. It is not necessary that I should discuss the various means of effecting this; let me, however, say a few words in reference to two which have more recently come under notice: these are—first, incision into the bladder from the perineum; and, secondly, paracentesis through the enlarged gland.

For the purpose of securing a more or less permanent channel for the escape of urine from the bladder, other than by the urethra, I must admit that, following the practice of Syme, and to some extent of Edward Cock, I have a decided preference for an incision through the perineum, on the twofold ground of safety and comfort. We have had numerous examples of the great benefit that cystotomy is capable of affording for bladder affection dependent on a large prostate, none perhaps more striking than the case narrated by Mr. Lund, on the memorable occasion of the meeting in London of the International Medical Congress. The paper closes with this observation: "I have thus placed on record this case, unique in its character, and interesting and encouraging in its results, with the hope that should a similar case occur to any surgeon now present, he will not hesitate to give his patient the chance of benefit from a course of procedure so simple in its nature, and so likely to be followed by temporary, if not permanent, benefit."[1]

I may be permitted here to submit to your notice a

[1] Trans. International Medical Congress, vol. ii.

method of puncturing the bladder through the enlarged prostate which has afforded very gratifying results. It consists in passing the trocar through the gland, and retaining it in the perineum, so as to afford a permanent as well as a convenient drain for the urine. I should have had more diffidence in commending this operation to your notice, had it not received the approval of our distinguished Associate, Prof. Gross, whose contributions to the surgery of the urinary organs are held in deservedly high repute on both sides of the Atlantic.

Though the primary object of cystotomy, as usually practised, is merely to place the bladder at rest by providing a continuous drain for the urine as well as the products of cystitis, it occurred to me, as it had already done to others, that it would be possible to extend this proceeding, with the view of removing those barriers to micturition which the hypertrophied gland so frequently presents.

It was to meet conditions such as these that Mercier introduced and practised division of the prostatic bar by means of a cutting instrument introduced along the urethra. This plan, though admirable in its conception, was open to the objection that in its execution it was necessarily uncertain, there being no means of surely ascertaining that the section was confined to the obstruction to be removed. On carefully considering the position of matters, as well as the proposals that had been made, it appeared to me more reasonable to attempt to divide the prostatic obstruction at the neck of the bladder from an opening made into the membranous urethra, than by means of instruments which had to traverse the whole length of the canal. I have recently brought under notice a case[1] in which I thought it desirable to explore the prostatic urethra from an opening made in the perineum, and through which I was enabled to divide, with precision, a prostatic barrier. The division of this portion of the gland was followed by complete restoration of the power of micturition, and has so far proved of permanent advantage.

The proceeding which I have thus put into practice seems first to have suggested itself to Mr. Guthrie, but I cannot find that he ever employed it. That it is not identical with the somewhat extensive incision of the prostate as for lateral lithotomy, which was practised by Sir William Blizard, is at once obvious. Its aim is to divide the obstruction—and the obstruction alone—by an opening so planned as not to expose the patient to undue risk, whilst, at the same time, it is capable of affording the greatest amount of room for manipulation by an extension of the incision, should this be found to be necessary.

I need hardly observe that a proceeding of this kind should be undertaken before the bladder has passed into a condition of confirmed and irremediable atrophy, otherwise, though we may succeed in removing an obstacle to the introduction of the catheter, our prospect of restoring the power of micturition will be as hopeless as it has proved to be under somewhat similar circumstances, where the operation of lithotomy has been undertaken.

It is impossible to avoid the conclusion, from their examination after death, that many atonied bladders might have been prevented becoming so by the timely removal of the obstruction by which a condition of permanent paralysis was induced and maintained.

The operative treatment of the enlarged prostate, when it obstructs micturition to a degree that cannot be met by judicious catheterism, is yet, I believe, open to considerable improvement.

Though the literature relating to either complete or partial excision of the prostate is very limited, there is much in it of promise. In one case, where I extirpated the whole gland for malignant disease, the benefit that followed far exceeded my expectations. It was that of a middle-aged man who, by reason of a carcinomatous prostate, was threatened with a speedy and painful death. I cut down upon the gland in the median line, and succeeded in enucleating it tolerably clean with my finger. I saw this patient eight months afterwards in very fair health, and quite able to go about his business. So far he has enjoyed an immunity from the symptoms which induced me to perform this operation, and though his disease is a malignant one, we have every reason to be content with the results obtained.

Then we have numerous examples where considerable masses of the prostate have been removed with very great advantage, in the course of operations on the bladder. Amongst these I would specially mention an important case by Mr. Bickersteth, and, more recently, another by Dr. John Ashhurst, of Philadelphia, in which the whole of an enlarged third lobe was successfully removed.

Cases such as these seem to favor the hope that operative surgery will be found capable of affording more relief to exceptional instances of this kind, and of extending to the large prostate the treatment which, in some degree, is applicable to other deep-seated growths.

In opening the Section of Obstetric Medicine DR. GRAILY HEWITT said: I take advantage of this opportunity to present a few observations on the question of food in obstetric and gynæcological practice.

There are certain terms in general use by the public, and by the profession, which are often employed in a vague, unmeaning, and indefinite manner—such as "weakness," "delicacy," etc.—whereby it is intended to designate a condition very frequently met with, but the essential nature of which is, as my experience has shown me, very imperfectly apprehended.

Another conclusion is, that the "weakness" is associated with and caused by a long-continued inadequate dietary, and that it is essentially what may be termed "chronic starvation." The rational view of the matter is, that this "weakness" is in the very large majority of cases preventable; that it should be regarded as the first stage of a serious possible disease; that it is mostly the result of chronic starvation, that it is curable, to a greater or less degree, in most cases, and that it is of the highest importance that it should be formally and carefully treated, as other diseases are, instead of being allowed to smoulder on until the constitution is thoroughly undermined, or until some ailment distinctly classified in our nosology makes its appearance.

It is a remarkable circumstance that so little as yet seems to have been done in the investigation of the effects of an insufficient dietary, looking at it from the quantitative point of view, although the effects of complete deprivation of food are well known. The popular impression is, that some people do not require so much food as others, and consequently, important diminution in the supply often escapes attention. I have to state the important conclusion, that a continuous insufficiency of food, or what may be termed a "chronic starvation," more or less intense in different cases, was found to have existed almost universally. Consequently, I have naturally been led to consider chronic starvation as a most important factor in disease, certainly in those classes of cases which have come more particularly under my notice.

It will be well to cite a few typical instances:

1. A young married lady, aged 24, consulted me for a severe retroflexion of the uterus, which appeared to have set in shortly after her first labor, some months previously. She underwent treatment for this affec-

[1] British Medical Journal, June 9, 1883.

[1] Trans. Royal Med.-Chir. Society, 1882.

tion; and, in the course of it, one day her mother came with her, and inquired of me why it was that her daughter had become liable to this affection. I replied that, in all probability, it was due to a previous condition of weakness, and insufficiency of meat as an article of food. "Yes," said the patient, "that is very likely true; for when I married, and could please myself, being very fond of sweets, I took little ordinary food during the first year, but lived chiefly on sweets, and ate little or no meat at all."

2. A young lady, aged 18, suffered much from menorrhagia, and her condition became finally one of complete inability for exertion. It was ascertained that this patient had lived regularly as regards her diet, but that her diet consisted principally of bread and puddings; and, having a marked aversion to meat, she had taken very little for two or three years previously; and it is the fact that I was afterwards consulted by two of her sisters presenting uterine symptoms, and with a corresponding history as regards the previous dietary.

3. Again, a young lady of very active habits, but who hardly ever took meat, became very ill after an unusual exertion, was affected with severe sickness, which went on unchecked for nearly a year, and was finally so ill that her life was despaired of. Here, the previous dietetic deficiency was most marked. The patient was finally completely cured, the sickness being found to be a reflex symptom dependent on a uterine displacement.

These cases might be multiplied *ad infinitum*.

I have been much interested in observing, also, the effects of previous insufficiency of food in apparently predisposing patients to attacks of puerperal septicæmia. In the cases of this disease which I have seen in consultation, I hardly recollect having seen a case where the patient so affected had, during the pregnancy, lived fairly well; and the worst cases have been those in which the patients fed badly and insufficiently during the pregnancy, and had been fed on a gruel diet after the labor was over. In cases where severe sickness during the early part of the pregnancy prevents the proper nutrition of the patient, the system is liable to become much impoverished, and an insufficient dietary may be, and often is, the preliminary to a dangerous childbed.

With very few exceptions, and those exceptions only tending to prove the rule, it is, I hold, impossible to find patients suffering from chronic uterine disease, who have not undergone, at some former period, what may be termed a starvation process, and careful inquiry generally elicits the fact that the quantitative deficiency in the diet extended over a considerable period. In many cases, the patients are found to be still under the influence of a deficiency in this direction, and to be "eating," as the expression is, "next to nothing."

The period of life during which quantitative deficiencies in the dietary are most common, is the two or three years immediately following the arrival of puberty. The girl is at school probably, her appetite is bad, or the food is not palatable, or is deficient in important particulars, or, as I have found in some cases, she eats little in order to keep thin; the strength fails, the appetite diminishes, and a habit of taking little is formed—particularly little animal food is taken. Three or four years of the most critical stages of life are thus passed, a time at which the body should be growing fast, and to maintain this growth in adequate vigor large supplies of nutritious material are required, instead of which the supply is far below the normal standard. The result is a general impairment of vigor and of vital action. On the generative organs, the effect is, as I have observed in numbers of cases, most decidedly injurious; and the effect, in most instances, is of this kind, that the tissues of the uterus lose their normal firm healthy consistence. The further result is that the pelvic organs as a whole, but particularly the uterus, undergo a subsidence in the pelvis; the uterus becomes liable to change of shape, and other alterations, more or less marked, in different cases. Slight exertions, slight accidents, or even moderate exercise, are, under these circumstances, liable to act most prejudicially on the softened and weakened contents of the pelvis. This is a faithful description of what I have observed to occur in very many cases. I refrain from pursuing the further history of patients so affected, as not falling within the scope of these remarks.

It is generally admitted by authorities on the subject of diet that nitrogen is the most essential of all foods, and that a certain amount, about three hundred grains, should be taken daily. In cases of chronic insufficiency of food, it appears that the diminution in quantity of food most frequently affects the nitrogen. Meat is the article of diet which, as a rule, is the source of the greater part of the needed amount of nitrogen. Milk is, of course, a most valuable alternative food; but, in these cases of absence of sufficient meat, we do not find it has ever been taken in any such quantity as to make up for the deficiency; and the quantity of bread consumed, even supposing it to be pure and of good quality, is in such cases entirely inadequate to supply the required quantity of nitrogen. I need not allude to the effect of deficiency in the other constituents of the diet. It is sufficient for my present purpose to show that the nitrogenous elements, while they are of all the most important, are those which are markedly absent in the cases now under consideration.

There are few observations bearing on the subject now under discussion which can be quoted from published works. Professor Voit (*Untersuchung. der Kost*, Munich, 1877) mentions an interesting fact. In a public institution, a home for girls, in which, he reports, the diet included an average quantity of one hundred and seventy grains of nitrogen only, the girls appeared healthy, but their muscles were found to be weak, and menstruation was found to be delayed in many cases until the sixteenth or seventeenth year.

Parker states that when this nitrogen is reduced to from 70 to 100 grains daily, the body gradually lessens in activity, and passes into a more or less adynamic condition, which predisposes to the attacks of all the specific diseases, especially malarious affections, typhus, and pneumonia, etc.

It must be assumed from what is known that if the minimum quantity of nitrogen—which, for the sake of argument, we may put as low as 250 grains in the case of a young woman—be not given, a condition of weakness will soon be induced, and with greater or less rapidity, according as the quantity of nitrogen falls much or little below this 250 grains *per diem*. Thus it is easy to understand why, in a year or two, with an average daily consumption of only 100 grains of nitrogen, for instance, important modifications of the nutritive processes are effected, whereby there is produced a direct predisposition to disease.

It is now some sixteen years ago, since I publicly discoursed on the importance of nutrition in the treatment of disease. I have made it the basis of my practice for some years past, and four years ago I described the condition to which I have now again referred as "chronic starvation." In the United States, Dr. Weir Mitchell has employed a system of rapid feeding, assisted by massage and electricity, for the cure of weakly, nervous sufferers, a system which has been found most successful, and Dr. Playfair has done good service in introducing Dr. Weir Mitchell's method

into this country. The method in question is essentially a rapidly acting means of introducing nutritive material into the system, and it is dependent for its success on the fact that the principle of the curative influence and action of food is adequately recognized.

It is probable that it will be found, on further investigation, that particular deficiencies in the dietary result in deterioration of particular tissues of the body. The field for inquiry in this direction appears to be a large one. Deficiency in the quantity of meat taken, certainly, according to my experience, appears to have an enormous influence in predisposing to uterine ailments; but, no doubt, other organs of the body also suffer from deficiency in this article of diet. And I have abundant evidence that it plays a very prominent part in predisposing to the serious wasting diseases so often fatal in this country. I refrain from dilating further on this particular point, because such discussion would in this place be inappropriate.

The conclusion which is obviously suggested by the foregoing considerations, is the necessity for a greater attention to the question of diet, in the bringing up of families, than appears at present to be given to it. We all know that health and a good appetite usually go together. But it seems to be too frequently the case that, when the appetite is absent, such absence is taken as a matter of course, and receives no notice. It results from what has been stated, that absence of appetite may lead to most serious results. It is not immediately dangerous, but it is the first step, possibly, in the downward course. A continuously bad appetite constitutes a grave condition, and should be seriously regarded.

SIXTH ANNUAL SESSION OF THE AMERICAN SOCIETY OF MICROSCOPISTS.

Held at Chicago, August 7, 8, 9, and 10, 1883.

(Specially reported for THE MEDICAL NEWS.)

TUESDAY, AUGUST 7TH, FIRST DAY.

THE meeting was called to order by DR. L. CURTIS, President of the Illinois State Microscopical Society, who welcomed the guests.

The President of the American Society, PROF. A. MCCALLA, in his reply, commented upon the growth of and increased interest taken in the Society. He suggested that a good part of the time be devoted to practical demonstrations, and mentioned that the Royal Microscopical Society of London had honored this Society by making its President a Fellow ex-officio; in response to which courtesy the Society should take action.

A recommendation by the Executive Committee, that the by-laws be so amended that any part of the proceedings might be published in any reputable journal, provided that due credit be given to the Society, was adopted; and numerous new members were elected.

In the afternoon, DR. F. M. HAMLIN, of Auburn, N. Y., read the first paper upon the

MICROSCOPICAL EXAMINATION OF SEMINAL STAINS ON CLOTH.

He found by observation that the method of Koblank, viz., to soak out the stains with water was unreliable, since the water disintegrates many, if not most, of the dried spermatozoa. Finding that most fabrics upon which spermatozoa are apt to be dried are more transparent than is usually supposed, he recommends the following procedure:

1. If the stain to be examined is upon any thin cotton, linen, silk, or woollen fabric, cut out a piece about one-eighth inch square, lay it upon a slide previously moistened with a drop of water, and let it soak for half an hour or so, renewing the water from time to time as it evaporates. Then with a pair of needles unravel or fray out the threads at the corners, put on the glass cover, press it down firmly, and submit to the microscope.

2. If the fabric is of such a thickness or nature that it cannot be examined as above, fold it through the centre of the stain, and with a sharp knife shave off the projecting edge thus made, catching upon a slide moistened with water the particles removed. After soaking a few minutes—say five to ten—the powdery mass will sink down through the water and rest upon the slide. The cover-glass may now be put on, and the preparation examined.

The latter plan serves as well for hairs, but great caution must be observed in cutting them, lest the portions bearing the suspected deposit fly away and are lost.

The stained cloth can be twisted and rolled between the fingers to a considerable extent, without destroying the dried spermatozoa.

In the discussion, various members stated their experience in the examination of seminal stains.

DR. GRADLE mentioned that spermatozoa can be stained like micro-organisms, which fact does not seem to be commonly known. Drops of the fluid containing them are dried into a film upon the cover-glass, drawn through a flame, and then stained with a solution of magenta, or some other aniline color, and washed. The spermatozoa attract the eye by reason of their intense staining.

DR. STILLSON asked at what age the secretion of seminal fluid began, which question could not be answered with precision.

A paper by MRS. SARAH WHITING, of Wellesley College, on

COLLEGE MICROSCOPICAL SOCIETIES,

was read by President McCalla, who stated that it was the first contribution to the Society by a lady. It dwelt upon the utility and practical arrangement of microscopical societies amongst college students, based upon six years' experience at Wellesley College.

A paper prepared by Prof. Simon H. Gage, of Cornell College, on *Cataloguing, Labelling, and Storing Microscopical Preparations*, was read in his absence.

In the evening THE PRESIDENT delivered the *Annual Address*, choosing as his theme the

VERIFICATION OF MICROSCOPIC OBSERVATION.

He dwelt upon the extensive application of the microscope for research in almost all sciences and its universal practical employment in numerous applied sciences. He insisted with great earnestness upon the necessity of increased accuracy in microscopic work, illustrating such model work by reference to Tyndall's researches upon spontaneous generation, which comprised over eleven thousand single experiments. The means he suggested for the verification of work were: the repetition of observations, including demonstrations at societies; the use of the camera lucida, and especially of micro-photography; a judicious application of the most varied media and reagents based on all recent researches in chemistry and physics; and the employment of improved lenses and accessory apparatus. All these suggestions were happily illustrated by well-chosen references to appropriate instances.

WEDNESDAY, AUGUST 9TH, SECOND DAY.

PROF. BURRILL, of Champaign, Illinois, spoke on *Preparing and Mounting Bacteria*, giving principally his own experience in staining dried films with aniline colors and the various details to keep the permanent specimens in good order.

DR. DETMERS, of Champaign, made some extemporary remarks on *Pathogenic Bacteria*. The points of special interest were that he had traced Texas fever in cattle to the existence of bacteria in the blood, and that he considered micrococci not a complete form, but merely one of the stages in the cycle of development of certain micro-organisms.

DR. GEORGE E. FELL, of Buffalo, spoke of the

CLINICAL ADVANTAGES OF OZONE AND ITS EFFECTS ON MICRO-ORGANISMS.

He referred at length to the observations of Dr. F. W. Bartlett, of Buffalo, who had used ozone clinically for many years by placing ozone generators in sick rooms in zymotic diseases. He claims to have observed in many cases a mitigation in the course of the diseases, as well as a checking of the spread of the contagious affection to other members of the families. At Dr. Bartlett's request, Dr. Fell had made some incomplete observations on the effect of ozone upon the growth of bacteria. For this purpose, he placed nourishing infusions partly into the ordinary air, partly into chambers in which the presence of phosphorus generated ozone, with the result that, in some instances, ozone hindered bacterial development, but not in all.

In the following discussion, various members gave their experience in attempting to limit the spread of contagious diseases by the use of volatile antiseptics other than ozone. No precise statements, however, were brought forth.

PROF. BURRILL pointed out the difficulties met with in the destruction of bacteria by antiseptics in solution, and suggested how unsatisfactory any attempt must be to annihilate bacteria or disease germs in the air; for all disinfectants proved harmful to animals in much less concentration than is requisite to affect the bacteria in the air.

DR. GRADLE endorsed these remarks and illustrated the difficulties of disinfection by referring to the care necessary to render a Lister dressing successful, stating that notwithstanding the abundant presence of carbolic acid, any slight negligence in the application of the dressing may open the way for infection of or through the wound. He suggested that the want of uniformity in the results of Dr. Fell might be due to the presence of different species of bacteria in different instances; for other observers had found difference in the resisting power of different species of micro-organisms to ozone.

DR. BLACKHAM presented the report of the committe on *Eye-pieces*.

In the afternoon, Dr. Blackham moved to reciprocate the courtesy of the Royal Microscopical Society, by electing its President an *ex-officio* member of this Society, which was adopted.

PROF. ROGERS, of Harvard University, read an elaborate paper upon *A Critical Study of the Action of a Diamond in Ruling Lines upon Glass*, which was followed by PROF. CHESTER'S (Hamilton College) paper on *Dry Mounting*.

In the evening, PROF. WALMSLEY, of Philadelphia, described and illustrated the details of *Photo-micrography*, and PROF. KELLICOTT, of Buffalo, described *Some Infusoria found on Cray Fish*.

THURSDAY, AUGUST 9TH, THIRD DAY.

DR. A. M. BLEILE, of Columbus, read a paper on

THE EFFECTS OF THE DIVISION OF THE VAGI ON THE MUSCLES OF THE HEART,

of which the following are the conclusions:

1. The fibres of the pigeon are more delicate and more friable than in the rabbit.

2. In young pigeons the cross-striation of the muscular fibres of the heart is indicated by the fine granules which might be mistaken for beginning degeneration.

3. Even in the adult pigeon granular fibres are occasionally found by the side of well-marked striated ones, the former being perceptibly narrower. Possibly the granular fibres are still embryonic and destined to replace the older ones as needed.

4. Recently Prof. Rincus has pointed out that the frog's heart has two sets of muscles—the outer one termed the mouth, and the other forming, by its arrangement into trabeculæ, a system of lacunæ in which the blood circulates. In our pigeon-heart preparations, blood-corpuscles seem to lie in interstices, possibly corresponding to the lucunæ of the frog's heart, and these corpuscles when altered by reagents might be mistaken for larger fat-granules.

5. After division of one vagus only, there follows in the heart muscle of the pigeon a fatty degeneration, about equally marked on both sides of the heart, and more plain the longer the animal has been kept alive after vagotomy.

6. After double vagotomy, the degeneration is better marked—*i. e.*, the fat-granules are large, and appear sooner than after one-sided vagotomy. Hence, we conclude that the vagus in the pigeon, as in the rabbit, carries trophic cardiac fibres—no other explanation accounting for the changes found—and that the influences descending by one nerve are sufficient to somewhat retard the degeneration on both sides, which would follow a double vision.

PROF. UPDEGRAFF, of Elmira, presented a paper defining *Some Hitherto Undescribed Animalculæ—Perehella-Annelida-Brachionus*, which was dissented from by Prof. Kellicott, who considered these forms but larval stages of other animals.

DR. CHRISTOPHER JOHNSTON, of Baltimore, made some remarks upon *A New Method of Polarizing Chemical Crystals*.

The sympathy of the Society was extended to Dr. R. H. Ward, of Troy, the first President of the Society, who has for a long time been hopelessly confined to his home with sickness.

PROF. ROGERS then presented an elaborate report of his measurements of the corrections of *A Standard Centimetre*, for which faithful work the thanks of the Society were extended to him. The standard of Prof. Rogers was accepted by the Society.

PROF. BLACKHORN, of Dunkirk, presented a paper on *The Relation of Aperture to Amplification in the Selection of a Series of Microscopic Objectives*.

In the evening the Calumet Club held a brilliant and well-attended microscopic *conversazione* and reception in honor of the Society.

FRIDAY, AUGUST 10TH, FOURTH DAY.

After the transaction of private business, the following were elected

OFFICERS FOR THE ENSUING YEAR:

President.—Hon. J. D. Cox, of Cincinnati.

Vice-Presidents.—Prof. T. J. Burrill, of Champaign, Ill., and Prof. W. E. Rogers, of Harvard University.

Executive Committee.—Prof. A. H. Chester, of Clinton, N. Y.; Dr. H. A. Johnson, of Chicago; and Gen. Wm. Humphrey, of Jackson, Mich.

DR. BELFIELD, of Chicago, showed some *Microphotographs of Lard*, pure and adulterated, to prove that the adulteration with tallow can be detected by the form of the crystals.

DR. V. S. CLEVENZGER, of Chicago, read a paper on the *Structure of the Brain in Health and Disease*.

DR. THOMAS TAYLOR, of Washington, forwarded a

paper on *Parasites in Fowls*, describing several varieties of mites found in the tissues of diseased animals.

DR. M. L. HOLBROOK, of New York, presented a paper on the

TERMINATION OF THE NERVES IN THE KIDNEYS.

By staining sections of kidney with chloride of gold he traced an abundant network of non-medullated nerve-fibres around the vessels, and could in some instances follow the ultimate fibrillæ into the cement-substance between the endothelial cells.

After the presentation of some additional papers and short addresses by the retiring and incoming Presidents the Association adjourned.

NEWS ITEMS.

THE CHOLERA IN EGYPT. — The mortality from cholera during the past week is undiminished. The disease has spread in Alexandria, and the mortality has decreased at Cairo.

The cholera mortality returns are as follows:

	Cairo.	Alexandria.	Total for Egypt.
August 7, . .	70	13	682
" 8, . .	78	12	627
" 9, . .	39	22	795
" 10, . .	37	22	926
" 11, . .	30	33	789
" 12, . .	11	21	407
" 13, . .	6	44	675

The Sanitary Commission has decided to remove the cordon around Alexandria.

IS IT CHOLERA?—English doctors now at Alexandria, who have had experience in India in cholera, as well as the Indian doctors, are reported as saying, that the disease now prevailing in Egypt is of a distinctly different character from the Asiatic cholera. Dr. Abatti, the personal physician of the Khedive, who accompanied His Highness to Cairo, is also reported to have said that his observations there, coupled with his experience in 1865, lead him to the conclusion that the cholera is not of the Asiatic type.

MEDICAL NOTES OF THE CHOLERA EPIDEMIC.—DR. J. MACKIE, Surgeon to the British Consulate at Alexandria, in his report of July 15th, to the British Foreign Office says: Since my report of the 9th instant [see THE MEDICAL NEWS, August 4th, p. 137] there has been a notable decrease in the mortality from cholera in the two villages which have suffered most severely—Damietta and Mansurah. The virulence of the disease, however, seems to continue unmitigated, if I can rely on reports which reach me from intelligent correspondents. The Board of Health at Cairo can give no information, and I am obliged to glean my information from other sources. Reports have been sent to me from Damietta and Mansurah of cases of death in from two to three hours, in which there had been neither vomiting nor diarrhœa. This seems to have occurred so often, that many inhabitants of those villages do not believe it is cholera, but some new disease. I remember, in my own experience and practice here in the cholera of 1865, many such cases. Though the number of victims in the villages first attacked has diminished, and no very severe outbreak has shown itself in any of the other villages, yet, from slight outbreaks occurring here and there, the disease shows a great tendency to spread.

Up to the present, the route it has taken is along the Damietta branch of the Nile southward, in the direction of traffic and prevailing wind, the only deviation from this line of progress as yet being Port Said, Menzaleh, Chibin-el-Koum, and Alexandria, and at none of these places, except, perhaps, Menzaleh, have there been more than a few cases. If this steady, though slow, progress continues, it is likely to find its way along the Nile to Cairo. The cases which have occurred at such places as Chibin-el-Koum and Port Said, may be accounted for by persons escaping from the infected villages, but I have no authentic information.

A great many of the soldiers forming the *cordons sanitaires* have taken the disease, notwithstanding the care taken, and stringent orders issued by the English officers. A letter from Colonel Sartorius of the 12th instant states: "One man Damietta *cordon* taken ill to-day. One officer, eleven non-commissioned officers Mansurah *cordon*, taken ill to-day. Talka *cordon*, one soldier, one officer, and nine men Samanoud *cordon* taken ill to-day, and two men dead. Chibin-el-Koum *cordon*, four taken ill and two dead three days ago." A paragraph, presumably authentic, in the *Egyptian Gazette* of the 14th instant, states "that it was found necessary to change the position of the *cordon* at Samanoud. In doing so, it was necessary to cross a native graveyard, where victims to the prevailing disease had only recently been interred. During the one night that the sentinels remained in the cemetery, they caught the malady."

The disease has now been raging in Egypt for twenty-four days in as fatal a form as the cholera of 1865, judging from the published bulletins; and, with the exception of the few cases at Port Said, most, if not all, of which the sanitary physician declares occurred in refugees from Damietta. The route across Egypt, from Asia to Europe, has been free. The origin of the outbreak at Damietta remains as obscure as ever. The Sanitary Board of Cairo has now announced, almost officially, its belief that the disease has been imported from Bombay. Others also express the same belief, holding the opinion that cholera invariably spreads from India, while those who are not bound by opinion, and ask investigation and proof, are inclined to find in the unsanitary state of the country where the disease first broke out, sufficient to account for an outbreak without importation. If the substance or germ necessary for the production of cholera can be produced or evolved from decaying animal matter, a local origin can easily be found at Damietta. What constitutes cholera, even in India, the means by which it is propagated, are now generally understood; but there must be a first beginning. What is it? If it has had its first beginning in unsanitary India, why not in unsanitary Damietta, with a river flowing through it supplying it with water filled with rotting carcasses of animals which have died of cattle-plague, four hundred of which, according to authentic information, have been fished out and buried since the disease appeared. The soil around is also the best that could be for the propagation of cholera, being loosely alluvial and fecal, and the water often nearly stagnant from the tidelessness of the sea. Under such circumstances, cholera could dwell and thrive. The question still is, how could it originate? The study of this epidemic will be epidemiologically, commercially, and politically of most intense interest, and the investigators will have to be independent men, free from all political influence, if truth is to be arrived at.

The Egyptian medical and sanitary administration has been showing great activity lately, and sent doctors, medicines and disinfectants to the infected villages; but sending one or two young medical men with disinfectants to disinfect an Egyptian village and stop cholera is like pouring a bucketful of water in the sea to flood a stranded ship. However well the work may look when published in the *Moniteur*

Egyptian, it is nearly practically useless. To disinfect an Egyptian town means destroying and reconstructing it—it is impossible to cleanse and disinfect it. And, moreover, I will undertake to say that nine-tenths of the men sent to do the work do not themselves appreciate the meaning and use, and method of disinfection. Sanitary habits and practice which will stop the progress of a zymotic disease are yet foreign to the countrv and to the nature of the people. Dr. Chaffey Bey reports from Damietta that the measures ordered are not carried out. Benevolent societies have been formed, chiefly by Europeans, to send aid to stricken villages, and have sent at their expense several European doctors who expressed their readiness to go. Many European doctors, experienced in cholera and epidemics, would gladly go to help authorities by their advice, but the difficulty of returning to their work here in case of need, without undergoing a lengthened quarantine, prevents them.

It would seem as if the *cordons sanitaires* have retarded the progress of the disease, and they may retard it till the country is supplied with good water by the rising of the Nile; but there are many reasons why the *cordons sanitaires* cannot stop its progress; where a case, for instance, happens in a village at some distance from an infected place, the people, or some of them, immediately take flight for fear of being shut in, and have escaped long before a *cordon* can be formed. Several cases have come before me, where people have arrived ill of fright, from villages where, perhaps, during the night, some one had died suddenly, and they had fled in terror from supposed cholera and to escape the *cordon sanitaire*. To make it more effective, it would be necessary to surround not only the villages infected, but the whole district.

Under date of July 17th, Dr. Mackie writes that the tendency of the disease to spread, notwithstanding *cordons sanitaires*, is very marked, and I am afraid there is now little hope of saving the country. It has now left the banks of the river and spread more in the direction of the chief line of railway. The *cordons sanitaires* are on a severe trial, and the result will prove whether they have been of any real use. If they prove to be a failure, it is to be hoped that they have had their last trial, here or elsewhere, as they are the cause of great panic and much hardship, which one would not desire to see inflicted if no good result is to be obtained.

Judging the rate of progress of this outbreak by that of 1865, when nothing was done, and the development and spread were so rapid that nothing could be done, it would seem as if the *cordons* have retarded the progress; but that they will stem the tide completely, I have no hope. The fatal nature of the malady is still its marked feature; in that it resembles real Asiatic cholera.—*British Med. Journal*, July 28, 1883.

FRENCH COMMENTS ON THE CHOLERA IN EGYPT.—M. FAUVEL, at the meeting of the Academie de Médicine of July 24th, fully discussed the present danger of an invasion of cholera. This danger, he said, is in inverse ratio to the distance from the infected point. At present Syria is in most danger on account of its proximity to Egypt, though the invasion of that country does not necessarily compromise Europe. But if Constantinople is attacked, the outlook for Europe is serious; for it will be in vain for Russia, Bulgaria, and Roumania to quarantine. Greece will, in all probability, be able to protect itself, isolated as it is in a great measure. Trieste is a dangerous point for Europe. The Italian Government has made every effort to prevent the importation of the disease, but these efforts are rendered partially futile by insufficient means of action, and the want of energy in the government agents. As to the maritime coast of France, the strict precautionary measures taken since the first outbreak of the disease rather preclude the idea of an attack from that quarter. But there are no measures for preventing an invasion from other European countries should they be attacked. The great question of the moment is, therefore, to prevent the invasion of any part of Europe.

The fact that England is in constant relation with the infected countries should not be lost sight of. The length of the voyage from Port Said to England is not less than fourteen days by steamer: but after fourteen days, if no cholera breaks out on board the vessel, there is no chance that it will be communicated by the passengers, and the English Government will cause all suspected cargoes to be disinfected. The history of cholera in England shows, that whenever that country has suffered from an epidemic, the disease was scourging the northern ports of Europe, and that England's invasion was from that source. Taking these things into consideration, we may affirm that Europe has a good chance of escaping an epidemic. One of the rules of cholera is that the more violent the epidemic, and the more rapid its extension, the shorter is its course and the sooner its extinction; and it is highly probable that the present epidemic will have died out in Egypt in five weeks more. The cholera in Egypt was an importation from India, and is the consequence of the suppression of proper measures for protecting the country, and at England's door lies the responsibility of its importation.

SANITARY PRECAUTIONS IN TURKEY.—The Sanitary Council has ordered that vessels arriving from Smyrna be subjected to observation for twenty-four hours in the Dardanelles. This order is due to the fact that a French man-of-war from Port Said disregarded the quarantine regulations at Smyrna and communicated with the town.

BRITISH PRECAUTIONS AGAINST CHOLERA.—The precautionary measures taken by England to prevent the introduction of cholera into her home ports are herewith presented as being of general interest to sanitarians in this country.

The Local Government Board has issued the following rules and regulations, to be enforced and executed by the Port Sanitary Authority for the Port of London:

For the purposes of this Order every ship shall be deemed infected with cholera in which there is or has been during the voyage, or during the stay of such ship in a port in the course of such voyage, any case of cholera.

I.—Regulations as to Detention by Officers of Customs.

Art. 2. If any Officer of Customs, on the arrival of any ship in the Port of London, ascertain from the master of such ship or otherwise, or have reason to suspect that the ship is infected with cholera, he shall detain such ship, and order the Master forthwith to moor or anchor the same in such position as such Officer of Customs shall direct; and thereupon the Master shall forthwith moor or anchor the ship accordingly.

Art. 3. While such ship shall be so detained no person shall leave the same.

Art. 4. The Officer of Customs detaining any ship as aforesaid shall forthwith give notice thereof, and of the cause of such detention, to the Sanitary Authority.

Art. 5. Such detention by the Officer of Customs shall cease as soon as the ship shall have been duly visited and examined by the Medical Officer of Health, or, if the ship shall, upon such examination, be found to be infected with cholera, as soon as the same shall

be moored or anchored in pursuance of Art. 10 of this Order. *Provided*, That if the examination be not commenced within twelve hours after notice given as aforesaid, the ship shall, on the expiration of the said twelve hours, be released from detention.

II.—Regulations as to Sanitary Authorities.

Art. 6. The Sanitary Authority shall, as speedily as practicable, with the approval of the Chief Officer of Customs of the port, fix some place within the jurisdiction of the Sanitary Authority where any ship may be moored or anchored for the purpose of Art. 10; and shall make provision for the reception of cholera patients and persons suffering from illness removed under Articles 13 and 14.

Art. 7. The Sanitary Authority, upon notice being given to them by an Officer of Customs under this Order, shall forthwith cause the ship, in regard to which such notice shall have been given, to be visited and examined by the Medical Officer of Health, for the purpose of ascertaining whether she is infected with cholera.

Art. 8. The Medical Officer of Health, if he have reason to believe that any ship within the jurisdiction of the Sanitary Authority, whether examined by the Officer of Customs or not, is infected with cholera, shall, or if she have come from a place infected with cholera, may visit and examine such ship for the purpose of ascertaining whether she is so infected; and the master of such ship shall permit the same to be so visited and examined.

Art. 9. If the Medical Officer of Health on making such examination as aforesaid (whether under Art. 7 or 8) shall be of opinion that the ship is infected, he shall give a certificate in duplicate, and shall deliver one copy to the Master and retain the other, or transmit it to the Sanitary Authority:

Art. 10. The Master of every ship so certified to be infected with cholera shall thereupon moor or anchor her at the place fixed for that purpose under Art. 6, and she shall remain there until the requirements of this order have been duly fulfilled.

Art. 11. No person shall leave such ship until the examination hereinafter mentioned shall have been made.

Art. 12. The Medical Officer of Health shall, as soon as possible after any such ship has been certified to be infected with cholera, examine all persons on board of the same, and all persons who shall not be certified by him, as hereafter mentioned, shall be permitted to land immediately on their giving their names and the places of their destinations.

Art. 13.. Every person certified by the Medical Officer of Health to be suffering from cholera shall be dealt with under any regulations that may have been made by the Sanitary Authority under Sec. 29 of the Sanitary Act, 1866; or, where no such regulations shall have been made, shall be removed, if the condition of the patient admit of it, to some hospital or place previously appointed for that purpose by the said Sanitary Authority; and no person so removed shall leave such hospital or place until the Medical Officer of Health shall have certified that such person is free from the said disease. If any person suffering from cholera cannot be removed from the ship, she shall remain subject, for the purpose of this Order, to the control of the Medical Officer of Health; and the infected person shall not be removed from or leave the ship, except with the consent, in writing, of the Medical Officer of Health.

Art. 14. Any person certified by the Medical Officer of Health to be suffering from any illness which such officer suspects may prove to be cholera, may either be detained on board the ship for any period not exceeding two days, or taken to some hospital or other place previously appointed by the Sanitary Authority, and detained there for a like period, in order that it may be ascertained whether the illness is or is not cholera. Any such person who, while so detained shall be certified by the Medical Officer of Health to be suffering from cholera, shall be dealt with as provided by Art. 13 of this Order.

Art. 15. The Medical Officer of Health shall in the case of every ship certified to be infected, give directions, and take such steps as may appear to him to be necessary for preventing the spread of infection, and the master of said ship shall forthwith carry into execution such directions as shall be so given to him.

Art. 16. In the event of any death from cholera taking place on board of such ship while so detained, the Master shall, as directed by the Sanitary Authority or the Medical Officer of Health, either cause the dead body to be taken out to sea and committed to the deep, properly loaded to prevent its rising, or shall deliver it into the charge of said authority for interment; and the Authority shall thereupon have the same interred.

Art. 17. The Master shall cause any articles that may have been soiled with cholera discharges to be destroyed, and the clothing and bedding and other articles of personal use likely to retain infection which have been used by any person who may have suffered from cholera on board such ship, or who, having left such ship, shall have suffered from cholera during the stay of such ship in any port, to be disinfected, or (if necessary) destroyed; and if the Master shall have neglected to do so before the ship arrives in port, he shall forthwith, or upon the direction of the Sanitary Authority or the Medical Officer of Health, cause the same to be disinfected or destroyed, as the case may require; and if the said Master neglects to comply with such direction within a reasonable time, the Authority shall cause the same to be carried into execution.

Art. 18. The Master shall cause the ship to be disinfected, and every article therein, other than those last described, which may probably be infected with cholera, to be disinfected or destroyed, according to the direction of the Medical Officer of Health.

DISINFECTION OF THE MAILS AT MARSEILLES.—A laboratory has been fitted up at Marseilles for the purpose of disinfecting the mails. It consists of a closet in which is a large table for receiving the contents of the mail sacks. The postal agents of the ship under observation carry the mail matter and sacks, previously unfastened, to the closet; they then retire to the ship or lazaretto. After having hermetically closed all the openings in the chamber, carbolized or chlorinated vapor is thrown in. After this the postal clerks of the city bring their mail, put it into the disinfected sacks, which are left for the postal clerks of the ship, and they carry away with them the disinfected mail. There is no communication between the two sets of clerks. Before being fumigated, the wrappers and envelopes of all the mail matter are cut through.

THE MEDICAL NEWS' Indian mail, just received, bears emphatic evidence of this antiseptic treatment, having been punctured through and through, in several places, by a broad, flat, and sharp instrument, and upon opening, it smelled strongly of chlorine.

CHOLERA HOSPITAL FOR PARIS.—The Director of Public Charities in Paris has asked the Municipal Council to grant a credit of 1,500,000 francs to be used in building a provisional hospital of 600 beds, in view of an attack of cholera. The Commission proposes—1. To give a sum of 400,000 francs. 2. To devote this sum to the construction of barracks, to contain 200 beds, with the necessary attendants and facilities for treatment.

CHOLERA PRECAUTIONS IN SPAIN AND PORTUGAL.—The Department of State has been informed by the United States Consul General at Lisbon, that sanitary measures have been taken by the Portuguese Government against the introduction of cholera. By a ministerial decree the Egyptian ports of the Mediterranean were declared infected by cholera, and all the Red Sea ports were declared suspected. The Spanish Government has imposed like vigorous measures upon vessels arriving from the English ports, and a portion of the Madrid press has advised that until like restrictions are imposed in Portuguese ports upon such vessels, an embargo be laid upon all persons entering Spain by land from Portugal. It is said that concert of action is contemplated by certain of the European powers as regards the continental ports of Central Europe, and that stringent measures are under consideration to restrict intercourse with infected ports.

CHOLERA PRECAUTIONS IN BERLIN.—The health officer of Berlin has issued the following instructions regarding the precautions to be taken to prevent the introduction of cholera into Germany:

The disinfectants used in ships must be; (1) a solution of pure carbolic acid in water (1–18). 2. Corrosive sublimate solution must be used for disinfecting the holds of ships and their contents. 3. Where these disinfectants cannot be conveniently used, steam, at a temperature of 280° Fah., must be employed for one or two hours. The different apartments of the ships, especially the cabins, berths, and the spaces between decks must be disinfected with carbolic acid solution. The decks, walls, and floors must be washed with rags previously soaked in the carbolic acid solution. The sick-rooms, and all vessels and utensils must be frequently washed and disinfected with this solution. Clothing must be washed in a disinfecting solution and then subjected to steam disinfection. 4. Where it is necessary to disinfect a person, he must be thoroughly washed with green soap, and must afterward be bathed. 3. Bodies of persons supposed to have died of cholera on board ship must be wrapped in cloths as soon as possible, and soaked in carbolic acid solution.

The disinfection of the holds of ships and their contents is to be carried out, with solution of corrosive sublimate, in the following manner: According to the quantity of bilgewater, one and a half to two and a half pounds of corrosive sublimate must be completely dissolved in from ten to twenty quarts of water. (For every thousand quarts of bilgewater two and one-half pounds of sublimate must be used.) Then, by means of a pump and a pipe running above deck the water must be pumped from the after part through the pipe to the forward part of the hold. It is of course understood that care must be taken, in case the hold is not connected throughout, that this washing must extend to the separate partitions of the bulk-heads, and a complete circulation of the bilgewater carried on. The sublimate solution must then be poured into the forward part of the hold and the pumping again carried out. On account of the poisonous nature of corrosive sublimate, all vessels containing it or its solution must be properly labelled, and after being used must be locked up. To know whether the disinfection has been properly carried out, and the water thoroughly impregnated with the solution, a strip of copper sheathing cleaned with emory-paper must be let down half-way into the water. If the disinfection is sufficient, there will form, at the expiration of about two minutes, a distinct gray coating upon the part of the copper strip which was immersed. If this coating does not form at the end of two minutes, the disinfecting solution is not sufficiently strong and must be strengthened and the pump-work gone over again.

After the bilgewater has been treated in the above manner with the sublimate solution, it must be left in the hold for twenty-four hours; at the expiration of this time the contents of the hold must be pumped out and an equal quantity of sea-water pumped in. In cases in which it is suspected that some of the old water still remains, this must be repeated at least three times.—*Berliner klin. Wochensch.*, No. 31, 1883.

GERMAN INVESTIGATION INTO CHOLERA.—The German government will send a scientific expedition to Egypt this week to examine into the origin, nature, and cause of the cholera now prevailing there, and to ascertain the best measures to prevent the spread of the disease.

QUARANTINE AT SAN FRANCISCO.—Fearing the introduction of cholera from China and Japan, the Board of Health has adopted a resolution that all Asiatic ports be declared infected, and that all vessels arriving be quarantined until the fullest examination be made.

YELLOW FEVER IN HAVANA.—There were twenty-nine deaths from yellow fever in Havana, for the week ending August 11th.

SMALLPOX IN NEW ORLEANS.—The mortality from smallpox for the week ending August 4th was thirteen, against twenty-one for the preceding week.

SMALLPOX IN MINNESOTA.—The smallpox is reported to be making terrible ravages among the Indians of Itaska county, Minnesota. In two small towns eighty-five deaths have been reported.

AMERICAN DERMATOLOGICAL ASSOCIATION.—The seventh annual meeting will be held at the Sagamore House, Green Island, Lake George, on Wednesday, Thursday, and Friday, August 29th, 30th, and 31st. Papers will be read by the following gentlemen: Dr. Piffard, Treatment of Acne; Dr. Hyde, A Study of the Coincidence of Syphilitic and Non-Syphilitic Affections of the Skin; Dr. Graham, General Exfoliative Dermatitis; Dr. Stelwagon, Impetigo Contagiosa; Dr. Robinson, Alopecia Areata; Dr. Duhring, 1. On the Value of a Lotion of Sulphide of Zinc in the Treatment of Lupus Erythematosus, 2. Report of a Case of Ainhum with Microscopic Examination; Dr. Atkinson A Case of Multiple Cachectic Ulceration; Dr. Sherwell, 1. Pseudo-Psoriasis of the Palm, 2. Malignant Papillary Dermatitis; Dr. Bulkley, 1. A Hitherto Undescribed Vegetable Parasite Found on the Human Skin; 2. A Clinical and Experimental Study on Pruritis; Dr. Van Harlingen, Experiments in the Use of Naphthol; Dr. Fox, A Trip to Tracadie.

THIRD INTERNATIONAL OTOLOGICAL CONGRESS.—The Committee of Organization for the Third International Otological Congress—Dr. Burchhardt-Merian (Bâsle), president, A. Hartmann (Berlin), L. Löwenberg and Ménière (Paris)—at a meeting on March 18th, decided to abide by the decision of the Second Congress to hold the session of the Third in Bâsle, Switzerland, the first week in September, 1883. The committee enlarged their number by the addition, among others, of Drs. St. John Roosa of New York, and Blake of Boston.

A SANITARY CONVENTION will be held at Muskegon, Michigan, on August 23d and 24th, under the auspices of the State Board of Health.

There will be sessions the first day at 2 P.M. and 8 P.M.; on the second day at 9 A.M., 2 P.M., and 8 P.M. At each session there will be addresses or papers on

subjects of general interest pertaining to public health, each paper to be followed by a discussion of the subject treated.

The objects of the Convention are the presentation of facts, the comparison of views, and the discussion of methods relating to the prevention of sickness.

Among the subjects of general interest which it is expected will be presented and discussed are the following:

Communicable diseases—scarlet fever, diphtheria, and smallpox.

Ventilation of residences and public buildings.

Food, and the sanitary regulation of markets.

Instruction in the public schools on the effects of alcohol and narcotics.

Adulterations and nostrums.

The disposal of refuse organic matter.

THE INTERNATIONAL MEDICAL CONGRESS, COPENHAGEN, 1884.—The eighth International Medical Congress will be held in Copenhagen from August 10 to 16, 1884. The General Organizing Committee, formed for the preparatory work, is composed of the following members, living either in or near Copenhagen: *President:* Prof. Dr. P. L. Panum. *Secretary-General:* Prof. C. Lange. *Secretaries;* Dr. O. Bloch, Dr. C. J. Salomonsen, and Surgeon-General Joh. Möller. *Honorary Treasurer:* Prof. Dr. E. Hansen Grut, besides the Chairmen of the Sections, viz.: *Anatomy:* Prof. Chievitz. *Physiology:* Prof. Dr. P. L. Panum. *General Pathology and Pathological Anatomy:* Prof. Dr. C. Reisz. *Medicine:* Prof. F. Trier. *Surgery:* Prof. Dr. Holmer. *Hygiene and State Medicine:* Dr. E. Hornemann. *Military Surgery and Medicine:* Director-General of the Medical Department of the Army, Salomon. *Mental and Nervous Diseases:* Prof. Dr. Steenberg. *Obstetric Medicine and Surgery, and Gynæcology:* Prof. Dr. Stadfeldt aud Prof. Dr. Howitz. *Diseases of Children:* Prof. Dr. Hirschsprung. *Ophthalmology:* Prof. Dr. E. Hansen Grut. *Diseases of the Skin and Syphilis:* Prof. Dr. Haslund. *Diseases of the Ear and Throat:* Dr. W. Meyer.

In order that the meeting of so many distinguished medical men, who, it is hoped, will attend on this occasion, may be as advantageous as possible, the Organizing Committee, following the example of the later Congresses, will communicate with distinguished men of different branches and of different countries, in order to prepare a programme. "This programme, as well as the rules, will be forwarded to those of our colleagues who we suppose take an interest in the work of the Congress, and who might be inclined to participate in it. In order that the programme may be ready as soon as possible, we shall be pleased if communications, referring to the work of the Congress, are sent to the Secretary-General before October 1 next, so that it may be possible for us to have regard to them in arranging the definite programme. The programme and rules will be forwarded, as soon as possible, to everyone qualified to participate in the Cougress, who within the limited time has announced to the Secretary-General his interest in the Congress, and his intention of participating in it; and, if possible, which section he intends chiefly joining."

HONORS TO MEDICAL MEN.—The Queen has conferred the honor of baronetcies on Mr. Prescott Hewett and Dr. Andrew Clark, and a knighthood on Mr. George Porter, of Dublin. The profession in Ireland remonstrated that a Surgeon of the Queen, in Ireland, should not have been the recipient of an hereditary title. Dr. Banks to whom a knighthood was also offered, in view of this expression of opinion of his colleagues in London, has felt it inconsistent with his professional duty to accept this mark of royal favor.

HEALTH IN MICHIGAN.—Reports to the State Board of Health for the week ending August 4, 1883, indicate that inflammation of the bowels, cholera infantum, remittent fever, typho-malarial fever, and whooping-cough have increased, and that diarrhœa has decreased in area of prevalence.

Including reports by regular observers and by others, diphtheria was reported present during the week ending August 4, and since, at sixteen places; scarlet fever at twelve places; and measles at eleven places.

Compared with the average for the month of July in the preceding six years, neuralgia and bronchitis were considerably more prevalent, measles, rheumatism, and tonsillitis were more prevalent, and membranous croup, whooping-cough, and remittent fever were less prevalent during the month of July, 1883.

OBITUARY RECORD.—At Albany on August 13th, in the fiftieth year of his age, JACOB S. MOSHER, M.D., Professor of Medicine in the Albany Medical College.

Dr. Mosher was born in Albany County on March 19, 1834. He entered Rutgers College in 1853, and in 1863 he received the degree of M.D. from the Albany Medical College. He at once tendered his services to the Government, and until the close of the war he served with the Army of the Potomac, and in the Washington Hospitals. He was Surgeon-General of the State of New York under Governors Hoffman and Dix, and from 1870 to 1876 he was Deputy Health Officer of the port of New York. At the time of his death he was a member of the Albany Board of Health, a Professor in the Albany Medical College, Dean of the Faculty of the College of Pharmacy, President of the Alumni of the Albany Medical College, and Physician to the Albany Hospital.

OFFICIAL LIST OF CHANGES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT, U. S. ARMY, FROM AUGUST 6 TO AUGUST 13, 1883.

MCPARLIN, THOMAS A., *Lieutenant-Colonel and Assistant Medical Purveyor.*—Relieved from duty in charge of Purveying Depot in San Francisco, California, to take effect September 1, 1883, and will then proceed to New York City, and relieve Assistant Medical Purveyor Ebenzer Swift of the charge of the Purveying Depot in that city. Assistant Medical Purveyor McParlin will transfer all funds and public property in his possession to Medical Storekeeper Henry Johnson, who, until further orders, will perform the duties of Acting Assistant Medical Purveyor at the Purveying Depot in San Francisco.—*Par. 5, S. O. 183, A. G. O., August* 9, 1883.

WOODWARD, JOSEPH J., *Major and Surgeon.*—Leave of absence granted, on account of sickness, by S. O. 34, extended six months.—*S. O. 179, A. G. O., August* 4, 1883.

BYRNE, CHARLES B., *Captain and Assistant Surgeon.*—Relieved from duty at Fort Craig, N. M., and assigned to duty at Fort Lewis, Colorado.—*Par. 3, S. O. 161, Department of the Missouri, August* 6, 1883.

LAUDERDALE, JOHN V., *Captain and Assistant Surgeon.*—Granted leave of absence for two months, to take effect on or about the 15th instant.—*Par. 2, S. O. 90, Department of the Missouri, August* 6, 1883.

BANISTER, JOHN M., *First Lieutenant and Assistant Surgeon.*—Relieved from duty in the Department of the Missouri, and assigned to duty in the Department of the East.—*Par. 5, S. O. 183, A. G. O., August* 9, 1883.

CARTER, WILLIAM F., *First Lieutenant and Assistant Surgeon.*—Relieved from duty in the Department of Texas, and assigned to duty in the Department of the East.—*Par. 5, S. O. 183, A. G. O., August* 9, 1883.

KANE, JOHN J., *First Lieutenant and Assistant Surgeon.*—Relieved from duty in the Department of the Missouri, and assigned to duty in the Department of the East.—*Par. 5, S. O. 183, A. G. O., August* 9, 1883.

OWEN, W. O., JR., *First Lieutenant and Assistant Surgeon.*—To proceed from Vancouver Barracks to Fort Walla Walla, W. T., and report to the commanding officer of the latter post for temporary duty.—*S. O. 101, Department of the Columbia, July* 27, 1883.